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ON VARIOUS CIRCUMSTANCES IMPORTANT TO BE ATTENDED TO IN PRESCRIBING MEDICINES.

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INDEPENDENTLY of a knowledge of disease and of its treatment, much of the success of the medical practitioner depends upon various circumstances connected with the form in which medicines are administered, and the causes which regulate their action upon the animal economy. In prescribing a medicine, even the best qualified for fulfilling the purpose wanted, it becomes necessary to take into consideration the following important circumstances, and to regulate it by them:—

1. The age of the individual;
2. The sex;
3. The temperament;
4. The habit;
5. Idiosyncracy;
6. Situation in life; and
7. Effects of disease on the system

before the dose can be properly determined upon, and as far as the medicine itself is concerned, the most agreeable form of administering it—whether it should be given alone, or combined with other ingredients, and how far these are likely to impede or facilitate its operation.

Attention to all these circumstances is necessary to prevent the errors which so often occur in drawing up prescriptions. For nothing distinguishes the well-educated medical practitioner, or tends more to the future recovery and comfort of his patient, than the nature of his prescriptions. Doses of medicine are not reducible to any general rule, from their general similarity of operation, or any other circumstance; they are, therefore, specific with regard to each other.

I will now proceed to make a few practical remarks upon each of the circumstances already alluded to, as affecting the action of medicines upon the animal economy, considering them in the order which I have laid down above.

AGE.—I commence this section with the table originally drawn up by Gaubrus, and copied from him by all successive writers to the present day, and which possesses no equal for perspicuity and correctness.

For an adult suppose the dose (of any medicine) to be 1 drachm;

Under 1 year will require ..	1-12th or 5 grains	
2	1-8th .. 8 grains	
3	1-7½th 10 grains	
4	1-4th ..15 grains	
7	1-3rd .. 1 scruple	
14	1-half.. ½ drachm	
20	2-3rds.. 2 scruples	
Above 21 the full dose	one .. 1 drachm	

65 the inverse gradation of the above.

From infancy to manhood, a larger dose of any medicine is required to produce its effects in proportion to the advance in life. From manhood to old age there is a similar gradation with regard to diminution of dose, though in a much less proportion than that which regulates the increase. In the two extremes of life,—childhood and old age, the animal economy is weak, and susceptible of all impressions, whilst during the intervening period, or what is termed adult age, it remains stationary. From infancy to adult age, therefore, the dose of medicine should gradually increase. During adult age it should remain the same, and this is what is given in all medical works as the standard dose; whilst from adult up to extreme old age, it must again gradually diminish in quantity, but not in so rapid a ratio as it increased from infancy to old age.

With regard to medicines, however, it is of consequence to observe, that there are many which do not act with greater violence in a large dose, than in one comparatively small. Ipecacuanha, as an emetic, is more certain in its operation when given in a large dose, than in a small, and is not more violent. The same may be said of aloes and others. In fact, almost all the medicines included under the classes of emetics and purgatives, come under this observation. In the action of an emetic or purgative upon the animal economy, it seems probable that that portion of the medicine which comes

into immediate contact with the internal villous coat of the intestinal canal, stimulates it to action, when the remainder will be thrown off or evacuated, without producing any further effect. From a large dose of such medicines, therefore, disagreeable effects are not to be apprehended. Whereas, when too small a dose is given, no effect is produced, the remedy remains in the intestinal canal, acting as an extraneous body, aggravating the symptoms which it was intended to relieve, causing the loss of valuable time. This remark applies to the generality of medicines.

Sex affects to a considerable extent the operation of medicine upon the animal economy. Although some women possess nearly as much bodily strength and vigour of constitution as the majority of men, yet the greater general delicacy and sensibility of the female frame at every period of life, require not only caution in apportioning the doses of active medicines which should be less than those ordered for men of the same age; but the medicines themselves should be such as are likely to fulfil the indications required without much violence. The state of the uterine system must be attended to in prescribing for females. Drastic purgatives, aloetic ones, and astringents, may be avoided during the flow of the catamenia.

Temperament affects the operation of medicines upon the animal economy. By temperament is understood a predisposition, derived from original conformation, to be affected in a more peculiar manner by external causes acting on the system, and much laborious investigation has been bestowed in distinguishing the different temperaments, or original conformation of body, and the diversities to which they give rise. It is undoubtedly true that persons of different temperaments are differently affected by the operation of medicines. Stimulants more readily affect those of a sanguine than those of a phlegmatic, temperament, and therefore smaller doses are required. In the phlegmatic also, the bowels are generally torpid, and require both descriptions of medicines, and such doses of them, to excite the proper peristaltic motion, as would induce either inflammation, or be followed by an alarming state of debility, were they administered to those of a sanguine temperament. Hence the necessity of attending to this circumstance in prescribing.

Habit or custom exerts a most important influence upon the operation of medicines. Persons addicted to the use of spirits, narcotics, and other stimulants, are less easily excited at an after period, both by medicinal stimulants and narcotics; and the same takes place with respect to purgatives. A knowledge of the habits of the patient, as far as the exhibition of all these is concerned, is absolutely necessary for the prescriber, many people being in the almost daily habit of taking these remedies without consulting the practitioner. In the first of these cases, larger doses of stimulants and narcotics are required to produce the ordinary effects of these remedies, but in the second a change of the purgative usually taken will generally be sufficient. In the employment of medicines which require to be long continued, the beneficial effect is soon lost if the dose be not increased.

In a few cases, the reverse of the above will be found to hold good, as, for example, with regard to the saline cathartics. As an example of the first of the cases alluded to, I may mention opium, one grain of which will produce upon an adult, unaccustomed to its use, all the effects of a narcotic, whereas ten grains will not do so at an after period, in the same individual, when long accustomed to its use. The first thing, therefore, that the medical attendant must do, upon being called in to a patient previous to prescribing opium, is to ascertain whether or not he be in the habit of taking that medicine, and to what extent. The dose must be regulated accordingly. With regard to the saline purgatives, as sulphate of magnesia, the dose requires to be diminished after long use; half an ounce at last producing effects equal to one ounce at first. The same remark applies to castor oil.

Situation in life has a considerable effect in regulating the action of medicines upon the animal economy. Individuals of the higher ranks—and this applies particularly to females leading a sedentary life, little exposed to air, exercise, or variations of weather, not compelled to gain their bread by their own industry, are much more susceptible of the action of medicine, and of course require milder ones, and in smaller doses, than individuals belonging to the lower orders. In other words, a female of the lower orders requires as strong medicines, and as large doses, to produce equal effects as a male belonging to the higher ranks, and *vice versa*. In the higher ranks I have always found the common aloetic pill sufficiently active as a purgative, whereas in the lower it has generally disappointed me, proving weak or inert, so that I have for many years past been obliged to give it up, under such circumstances having recourse to the compound colocynth pill, which has always answered my expectations. The practice of dispensaries, hospitals, &c., and that confined to the lower ranks of life, as far as regards medicines, is considerably different. Upon a similar principle the practice of different

countries varies greatly, and that which has been followed by the practitioners of a place for ages, must always be superior to the innovations of the stranger. The population of France, Italy, &c., enjoying a settled, warm, southern temperature, and living principally upon fruits, vegetable diet, &c., cannot bear the active medicines and practice adapted to the inhabitants of Great Britain, Russia, and other northern regions, exposed to every variety of weather, living upon animal food, and accustomed to the use of spirituous liquors. A wandering life passed in these countries firmly establishes the accuracy of this remark upon my mind. The treatment of syphilis in southern and northern latitudes—its yielding without mercury in the former, whilst it absolutely requires its active ministration in the latter for its extirpation, may be given as illustrations of the above.

Disease exerts an influence on the action of medicine in the animal economy very important; the susceptibility to external impression and to action being much varied in morbid affections, and the operation of medicines of course being modified by such variations. The state of susceptibility being in general different when it varies much from that healthy standard, the doses of the medicine administered are regulated accordingly, and thus it is obvious does not admit of much general observation, as being entirely dependent on the nature and state of disease. I may mention, however, two striking extremes. In the diseases arranged under the class *febres* and *exanthemata* of Dr. Cullen's arrangement, all characterised by febrile action, the animal economy is remarkably irritable, and susceptible to stimuli; the mildest medicines, and in the smallest dose, produce powerful effects. In the diseases arranged under the class *neuroses*, of the same arrangement,—mania, melancholia, hypochondriasis,—all characterised by stupor and insensibility to stimulants, the most powerful medicines, and in the largest doses, are required to produce even moderate effects.

Besides these guides now mentioned in the administration of medicines, the choice of the practitioner must occasionally depend upon the circumstance of the patient being more or less immediately under the practitioner. Then, if the patient can be seen every day, or frequently, by the practitioner, the most active medicines which the nature of the case requires should be chosen. But if he cannot be seen, or is not resident in the same place, the practitioner should choose a remedy of the same class, but less likely to have a violent effect. Thus in prescribing for intermittent fever, where a patient cannot be visited every day or oftener, the Peruvian bark should always be preferred to the arsenical solution. In dropsical complaints, the supertartrate of potash to the digitalis or elaterium.

In apportioning the dose of any very active medicine, it is of the greatest moment to determine the relative degrees of power between the system and the remedy; and to know to what extent the latter is likely to be carried consonantly with the power of life to resist it. Thus, after a patient has been exhausted by protracted or severe suffering, a dose different, or milder than one at the commencement of the disease, becomes necessary.

The variable activity of a medicine should also be appreciated, and the practitioner would act judiciously, if he were to reduce the dose, should it be a very considerable one, whenever a fresh parcel of a medicine is commenced, especially of the powders of active medicines liable to deterioration from being long kept; those of cicuta, digitalis, &c.

The time of day at which remedies should be administered, likewise deserves attention. Evacuating medicines ought to be administered late at night or early in the morning. It would seem that during sleep the bowels are not so irritable, and consequently not so easily acted upon, which allows time for the ready solution of the substance. The same observation applies to alterative and other medicines, which are liable to suffer from vexatious irritability of the bowels. It is on this account eligible to exhibit guiacum, pil. hydrargyri, &c., when they are not intended to purge, at bed-time. On the other hand, when the effects of a medicine are likely to be lost by perspiration,—as is the case with diuretics, many of which are by external heat changed into diaphoretics—it becomes a question with the judicious practitioner whether he cannot select some more favourable period for their exhibition. Medicines which are in the form of a pill, as they are liable to be long kept in apothecaries' shops, and of course to become remarkably hard, should generally be administered at bed-time, to give time for the gastric juice and other secretions of the intestinal canal to complete their solution. Under these circumstances they will pass through the whole tract of the bowels as inert bodies, disappointing the intentions of the practitioner and patient. Aloes administered in *powder* operates in from two to three hours; given in *pill* it requires from eight to twelve hours to produce its full operation.

In Fevers it is of importance to consult in all respects the quiet and comfort of the patient. Dr. Hamilton, in his admirable work on purgative medicines, justly observes that on this account the exhibition of purgatives should be so timed that their

effects may be expected during the day. In some cases the time of administering the remedy must be regulated by the stage of the disease. In fevers a dose of opium will either increase the heat of the fever, augment the thirst or restlessness, or occasion tranquillity and sleep, according to the temperature of the body at the time of its administration. For this reason Dr. Currie advises us not to give the evening dose of opium in typhus fever till very late, or about one or two o'clock in the morning, when the heat is subsiding and moisture coming on.

EMETICS, administered for the cure of the slighter cases of pyrexia, should be given in the evening, as their operation has a tendency to sleep and diaphoresis, which it is useful to promote. Remedies that require to be absorbed will probably be more efficient in the morning after sleep. The old custom of giving medicines in the morning, fasting, is not quite so absurd as some modern practitioners have been led to suppose. A weak or mild medicine, taken two hours before breakfast, upon an empty stomach, will operate equally powerfully as a much stronger one will if taken at a later period, or upon a full stomach. Every practitioner must be struck by the truth of this remark, when administering mineral waters, as those of Cheltenham or Harrowgate.

THE INTERVALS between doses of medicine must be regulated by the nature of the remedy, and that of the object which it is intended to fulfil, and whether it be desirable or not that the last dose should support the effects of the preceding; or whether there be any fear of a reaction, or collapse, taking place after the effect of one dose has subsided, unless immediately repeated. Thus the effects of diffusible stimulants, ammonia, or of ether, are very evanescent; they should therefore be repeated at short intervals. The same may be said of diaphoretics. We ought not to allow the periods between the doses to be so remote as to occasion any striking abatement in the impression. So opium, when stimulant operation is required,—as in diseases of debility, fevers of the typhoid kind, &c., should be given in small doses at short intervals, so that it may produce a uniform and regular state of excitement. But when the object is to mitigate pain, allay irritation, and produce sleep, it ought to be exhibited in full doses at distant intervals. There is a caution also which it is very necessary to impress upon the practitioner, respecting the power which some medicines possess of accumulating upon the system. This is notorious with regard to lead and mercury. Dr. Withering has observed that the repetition of small doses of digitalis at short intervals, till it produces a sensible effect, is an unsafe practice, since a dangerous accumulation will frequently take place before any signals for forbearance present themselves.

From a COMBINATION of medicines, beneficial and mild effects result, which would not be produced by any of them given separately. From two distinct emetics, combined together, or from two distinct purgatives, an effect results, which is far more certain and mild, than from either of the two administered separately; and the same holds good with other medicines.

Besides the causes now detailed, affecting in a very material point of view the action of medicine upon the animal economy, and which ought to be minutely attended to by every medical practitioner, it is necessary to take notice of a few others not less important, affecting the vegetable and animal world. There are SOIL, CULTIVATION, CLIMATE, SEASON, AND ADULTERATION. There can be little doubt but that soil, cultivation, climate, and season, may very materially influence the active properties of a medicinal plant; while the two latter of these causes (climate and season) may as essentially change the type and character of a disease, and modify the vital susceptibility of the patient. This must be admitted to its full extent, or it will be extremely difficult to explain the contradictory and even opposite opinions, and to reconcile the conflicting testimonies of the physicians of different countries, with respect to the efficacy of the same remedies in similar diseases.

THE INFLUENCE OF SOIL may be exemplified by many well-known facts. Thus, strong-smelling plants lose their odour in a sandy soil, and do not again recover it by transplantation into a richer one. Assafœtida varies much, according to station and soil, not only in the shape of the leaves, but in the peculiar mucous quality of the juice; and Dr. Woodville states, that it is frequently so modified, that the leaves are eaten by goats. In Russia, the rhododendron chrysanthemum, a celebrated remedy among the peasantry for rheumatism, syphilis, &c., varies in its effects very materially, according to the *natale solum*. That product in a certain spot has proved uniformly narcotic; that in another cathartic; while a sense of suffocation has been the only symptom occasioned by a third. Every medical practitioner knows that the wild digitalis, or fox-glove of the sandy hills ground, the valeriana officinalis, &c., possess medicinal qualities far more energetic than those of the low-ground production, or those of our gardens.

CLIMATE produces a powerful impression upon vegetable and animal life. It is probable that in some southern countries, some

vegetables enjoy more energetic properties than in northern climates. Senna, by transportation from Arabia into the South of France, assumes a morbid change in its physiognomy and virtues. The leaves are more obtuse, and its taste is less bitter and nauseous, than the pointed leaved variety, whilst its effects will be found to be less purgative. Can a more striking proof of this fact be adduced, than the well-known effect of perfumes at Rome! The inhabitants are unable to encounter the strong scent of flowers in that climate, without experiencing a sensation highly oppressive, and which, in some cases, is even succeeded by syncope. Dr. Harrison, who resided some time in Italy, states, that he has no difficulty in asserting, that narcotics act with greater force even in small doses, at Naples, than in England. The extract of hyosciamus, when given to the extent of three grains thrice a day, produced in two patients temporary amaurosis, which disappeared, and again recurred on the ultimate suspension and administration of the medicine. And it deserves particular notice, that these very patients had been in the habit of taking similar doses of the same remedy in England, without any unpleasant effect. Now that this depended rather on an increased susceptibility of the patient in the warm climate, than on any increased power in the remedy, is unquestionable, since the extract which was administered in Italy had been procured from London. I treated several cases of epilepsy in Italy with the nitrate of silver, and with complete success, whilst in England I have certainly not met with the same fortunate results. At the baths of the island of Ischia, the most prevalent diseases were effects arising from the abuse of mercury. Hence we may fairly conclude, that the metal is more active in its effects there, than in our own country. "The doses of medicines," says Dr. Harrison, "as seen in the prescriptions and works of English physicians, excite universal astonishment amongst the faculty of Italy." Medicines act differently on the same individuals in summer, in winter, and in different climates.

Cultivation.—Of all the circumstances that produce change in the aspect, as well as in the virtues of the vegetable creation, the most conspicuous is cultivation, which may either destroy the medicinal properties of a plant, or raise in it new and most valuable qualities. Cultivation converts single into double flowers, by developing the stamina into petals,—a change which in many cases destroys their efficacy, as in the anthemis nobilis, or camomile; for since all the virtues of the plant reside in the disc florats, it is of course greatly deteriorated by being converted into the double flower variety. Many other instances might be mentioned.

Adulteration deserves to be ranked amongst the most powerful causes which have operated in affecting the reputation of many medicinal substances. The Peruvian bark fell into total disrepute in the year 1779, from its inability to cure the ague, and it was afterwards discovered to have been adulterated with bark of an inferior species. Sydenham speaks of the adulteration of this substance before the year 1678. He tells us he had never used to exceed two-drachm doses of cinchona in the cure of any intermittent, but that of late the drug was so inert, rotten, and adulterated, it became necessary to increase the dose to one, two, or three ounces. "Very few practitioners have any idea (says Dr. Paris of London, who has paid particular attention to this subject) of the alarming extent to which the nefarious practice of adulterating medicines is carried on. There can be no doubt that it has been carried on in all ages, but the refinements in chemistry have enabled the manufacturer of the present day not only to execute this fraud with greater address, but, unfortunately, to vend them with less chance of detection. It will scarcely be accredited, when I affirm that many hundred persons are supported in London by the art of adulterating drugs; besides a number of women and children who find employment and excellent profit, in counterfeiting cochineal with coloured dough, isinglass with pieces of bladder, and the dried skin of soles; and by filling up with powdered sassafras the holes which are bored in spice and nutmegs, for the purpose of plundering the essential oils.

In prescribing medicines, the principal object to be kept in view is simplicity, and this was greatly neglected in the prescriptions of former days, and is even so still in those of surrounding countries. This is strongly remarked by Dr. Boyce in his Sketch of Medical Knowledge at Constantinople, recently published. Dr. Paris informs us that he was told by a medical practitioner in the country, that the quantity, or rather complexity, of the medicine which he gave his patients, for there never was any deficiency in the former, was always increased in a ratio with the obscurity of their cases. "If," said he, "I fire a great profusion of shot, it is very odd if some of them do not hit the mark!" Sir Gilbert Blane, in his *Medical Logic*, has given us a similar anecdote. A practitioner being asked by his patient why he put so many ingredients into his prescription, is said to have answered, more facetiously than philosophically, "In order that the disease may take which it likes best."

With regard to the directions in a pre-

scription how medicines are to be taken, they ought always to be written distinctly in the vernacular (English) language. In prescriptions, perspicuity is our first object; it is not here that a medical man is called upon to display his learning, and by clearness,—by writing the directions distinctly—he puts it out of the power of any individual to injure his reputation, or to endanger the safety of the patient.

Edinburgh, March, 1832.

OBSERVATIONS ON

THE INFLUENCE OF FEAR

IN PREDISPOSING TO DISEASE.

By James Clarke, *Esq., Assist. Surg., R.N.*

The human body, it is evident from repeated observation, possesses at different periods of life, greater or less powers of resistance within itself, to the operation of those diseases excited by means of atmospheric influence.

In whatever mode a vitiated atmosphere has the effect of lighting up disease in the body, of this we are certain, that a continued and modified exposure to its influence, deadens its power, and in a peculiar manner having impregnated the constitution, renders it free from attack.

A robust state of health is by no means necessary to a person undergoing this trial; although, at the same time, absolute debility is to be avoided. How often do we see nurses, who in general do not enjoy good health, engaged, daily, attending the most fatal cases of typhus, small-pox, and other acknowledged contagions, while a casual visitor in rude health falls a victim to the pest! Often does it happen that a delicate female may attend on a relative during the whole range of a fatal case of typhus without experiencing any bodily illness, but the moment the object of her hope has died, her mind sinks within her, and the deadly poison works upon her harassed frame.

In the operation of miasmata on the system, a great deal I conceive must be taken into consideration, with respect to mind and body. In the case of a nurse or physician having a comparative immunity from contagion, it is evident, as regards the operation of all poisons on the body, aerial or otherwise, that habitude to them materially weakens their force. Let us add, that from constant intercourse with the sick, their minds are perfectly free from alarm, and no useless fears prey on their nervous system. In accordance with this rule, an infant will be found uninjured sucking the breast of a mother dying of plague. When a stranger, perhaps in rude health, visits a patient from choice or necessity, labouring under a contagious fever, from the want of habitude to scenes of this description, he naturally feels an awe, if not actual fear, come over him. This of course must render his body more susceptible to the influence of the morbific cause, and it is no wonder if he imbibes the contagious miasm.

Again, on the other hand, we have seen the wonderful influence of hope in preserving the body secure during the intercourse with the sick, and the consequent infection on its being lost. There can be little doubt that during the panic of an epidemic disease, a great many are rendered susceptible by the influence of fear and other depressing passions. This is more particularly the case on board of ships, where the healthy and sick are necessarily nearly approximated.

That the operation of a morbid poison may be called into effect by dint of mental depression, or protracted fear, I have little doubt. And I think the following circumstances, which occurred on the coast of Africa, will tend to confirm it. At the same time I do not mean to ascribe all to the operation of the above cause, yet I do so in a great part, as the crew were constantly under my eye, and dissipation would have been immediately detected.

H. M. *Black Joke*, tender to H.M.S. *Sybille*, formerly employed in the slave-trade, was purchased in 1827 for government on account of her sailing qualities; from that period up to August 1829, she never lost a man by fever, and was remarkably healthy. No alteration had been made with regard to her slave-deck, which was composed of moveable planks, under which were stowed the wood and water. This deck was about four feet and a half from the beam; but the ventilation was complete. In the month of August 1829, H.M.S. *Sybille* had contracted a malignant yellow fever, it were said, at Fernando Po; about this period the *Black Joke* received five men from her in lieu of smugglers, whose period of servitude had expired. These men were reported healthy; but one of them who had slept on deck all night, which was generally the custom of the crew of the *Black Joke*, was taken ill next day, and died on the fifth with black vomit. All his clothes were buried, and the vessel was fumigated and white-washed. Nine days elapsed, when three cases were put on the list, and a second interval of thirteen days, when the fever fairly broke out. It continued during seven months, and attacked seventy-seven persons, of whom twenty-seven died. During this period the greatest possible de-

spondency and fear existed amongst the seamen, so that many would never go on the lower deck if they could avoid it, as they said there was contagion in the hold. A number of the native Africans employed on board were also attacked. Whilst things were in this state, we were ordered to the island of Ascension to receive refreshments, and for the benefit of change of air and exercise.

In consequence of losing two men with fever on our passage down, we were placed in quarantine.* The fever which had ceased for a few days again broke out, and it was finally agreed that sick and healthy should leave the vessel and be kept in quarantine on shore. During this period the hold of the vessel was cleared out by Kroomen (natives of Africa), when an immense quantity of black fœtid mud was found between the lining and plank. None of these blacks took the fever, but a robust young man, who had requested permission to superintend the work, was taken ill and died with black vomit on the island. After this there were no fatal cases, and the fever speedily vanished.

From the preceding history, there is nothing I can see to lead us to suppose that the cause of the fever (viz. the miasm generated from the fœtid mud in the hold) acquired unwonted virulence just as the first patient was received, for there was no visible alteration in the weather observed. The weather being in general dry, at times foggy, and the thermometer ranging from 84° to 90° in the shade. The vessel was almost constantly at sea, cruizing from latitude 6° 30″. N., to latitude 7° 15″ S. That the hold was in the same state before the first patient was received, I am disposed to believe. But the death of a person received from a vessel having yellow fever on board, excited very great alarm amongst a crew hitherto very healthy, fitting their bodies for the receipt of the miasm generated in the floating marsh over which they lived. The content and cheerfulness consequent on the total cessation of fever after the removal of the exciting cause, give countenance to the idea. As to the contagious quality of yellow fever, I am inclined to deny that it is contagious *in se*; but I have little doubt, like other fevers, it may from accumulation, such as patients crowded together, want of ventilation, and cleanliness, acquire that property.

It must be remarked, that the vessel was never out of the latitudes where yellow fever holds its reign; consequently change of atmosphere had nothing to do with the cessation of the fever.

* The effect of this order on the minds of the crew who had promised themselves so much enjoyment and relaxation may be easily conceived.

TREATMENT OF PULMONARY CONSUMPTION;

MORE ESPECIALLY WITH DIGITALIS.

To the Editor of THE LANCET.

SIR,—The prevalence of phthisis pulmonalis, or consumption of the lungs, in Great Britain as compared with Ireland, induces me to offer a few observations respecting this disease and the modes of treatment. Highly approving of the utility of what I have seen, and the plan and liberality of your publication, I have the honour to be, Sir, your most obedient servant,

THOMAS WOOD, M.D.

Cork, March 17, 1832.

As the chief causes, termed predisposing and exciting, are both obstacles to recovery, my first attention to some of them may not be considered irrelevant.

They are, First, constitutional; a form of body distinguished by narrow shoulders, a small and compressed thorax, with a temperament not unfrequently connected with scrofula, and marked by a clear and florid complexion.

Secondly, among exciting causes, I may notice the prevalence of the east and north-east winds blowing, many days of the year, from frigid continental regions.

Thirdly, a sudden vicissitude of weather.

The difficulty in the cure of phthisis was ascribed by an able physician and a good anatomist, the late Professor Rutherford of the University of Edinburgh, to the bole of the pulmonary artery being too large for the contracted size of the chest. Yet we know that nature often accommodates herself to malformation, that some chicken-crested persons have escaped phthisis, that the complaint is rare, if it occur at all, in tanyards, and that some, strongly predisposed to the disease, have been, during their abode in certain latitudes, not only free from cough, but have enjoyed health, and yet soon after returning to their native countries have contracted the consumptive cough, and died.

With regard to the usual modes of cure I mean to animadvert freely, not from any presumptuous motive, but with the hope that such a freedom may tend to elicit some information from a more competent source.

The objects in view are—1. To lessen the inflammatory diathesis, with the intention of assuaging the cough, and preventing the formation of ulcers. 2. To heal them when formed.

The common treatment with these designs is to abstract blood repeatedly in

small quantities, and to use mild purges. Nature, however, abhorring even a partial vacuum, the blood lost is—*ceteris paribus*, again supplied in a few days, and during that restoration its momentum or tendency is chiefly directed to the comparatively weak or injured parts of the organ affected. For a similar reason, blisters, although they often relieve, do not effectually prevent, a determination of blood to the diseased site. To diaphoretics, though mild, the objections are, debility consequent upon their use, susceptibility to cold, and their frequent failure. Emetics, composed of sulphate of zinc or copper, are occasionally useful. Long continued, they, though of mild effect, probably agitate the delicate bronchial and general systems too much; they likewise seem to obstruct that salutary medium which should subsist between the necessity for the absorption of morbid matter, and the undisturbed retention of food for animal support. It may at least be said of the topical application of dilute acetic acid, and of the internal use of nitre, that they cannot be relied on.

The inhalation of the vapour of hot water is often beneficial; of its impregnation with narcotics I have had no experience, and am equally ignorant of the effects of factitious airs. The prussic acid was tried in Youghal. I know not the number of cases, but certainly in one, far advanced, the lady died.

In the internal use of conium maculatum for the cure of consumption I have not much faith. I had it applied in the form of poultice to an injured mamma, until the nervous and sanguineous systems became for a considerable time sensibly affected, but without any change in the indurated gland, which, however, afterwards yielded to mere topical heat, communicated by a succession of heated tiles. I hence infer that in some, not improbably in many, instances of local applications of some remedies in repute, heat, or caloric, is the sole agent engaged in discussing such obstructions. Usually as a dernier resource, and with a slight prospect of cure, phthisical patients have been recommended to warm climates. As a prophylactic I think unfavourably of the practice, and when the disease becomes established, and especially in the last stage, I consider the removal to southern regions a speedier transition to the grave.

The preference given to new remedies or formulæ is often proved by experience to be a natural weakness. I therefore beg to submit to superior judgment the reasons which have induced myself to prefer to any other medicine as a basis, a remedy whose peculiar fate it has been to be often recommended and often discarded. This remedy, praised by some both in scrofula and phthisis, is *digitalis purpurea*.

Conceiving it to be the duty of every man to rescue from obloquy a useful truth, and not only to restore but to raise to a just estimation its real value, I venture upon the rather novel task of endeavouring to contribute a share in vindication of the unjustly aspersed character of this valuable plant.

Cautious of its ill effects after long use, upon the appetite,* and of its unnecessary violence, in phthisis, on the nervous and sanguineous systems, when administered in large quantities, I first attended to the dose; then (recollecting the effect of spirit of lavender in opium; in diminishing the injurious power of the latter on the stomach) to its compound, which I directed with a view to alleviate the irritated state of the lungs, and to promote absorption without causing debility. The medicine thus combined and administered in small doses, produced the following effects without any sensibly disagreeable sensation upon the stomach or nervous system:—

1. Increased action of the kidneys.
2. Reduction of pulse.
3. Absorption from the lungs.
4. Mitigation and removal of cough.
5. Improvement of appetite.
6. Cessation of night sweats.
7. Restoration of health.

This remedy has been tried in various pulmonary affections; it has, in several instances, arrested those symptoms which probably would have ended in phthisis; and in one consumptive case, which originated from cold and moisture, the hectic exacerbations were twice removed, the second attack having been caused by the effects of superstitious motives, and the consequent omissions of medicine. Aware, however, that symptoms of hectic fever sometimes appear, independent of the absorption of pus, I would not affirm, though I believe (the complaint having commenced a year and a half before I saw her) that they arose from the purulent matter of tubercles. Candour too demands the avowal, that in the advanced stage of the disease, which is accompanied with colliquative diarrhœa and aphthæ, I have had no opportunity of ascertaining the effects of this formula. Yet even in this, my hope, feeble as it should

* A clerical patient, afflicted with cough, pain of chest, and nocturnal sweats, a fortnight before I saw him, states that those affections were removed, and his lost appetite restored, in one week after his using this medicine; but believing that his immediate family and himself were predisposed to phthisis, and dreading a return of his complaints, caused by sleeping in a damp bed, he used his medicine thrice a day above three weeks, but had experienced no diminution of appetite. He now enjoys perfect health, though, in the discharge of ministerial duties, he has been since frequently exposed to night air.

be, in consequence of the waste committed in the lungs, and of the inefficacy perhaps of all medicines as yet tried by many celebrated members of the faculty, would rest with more confidence upon it, than on any remedy of which I have heard.

Within my neighbourhood, a healthy rural situation, a few miles distant from the city of Cork, consumption of the lungs is not a common disease. Many of the peasantry, too, ascribe its advanced state to a fairy stroke, an effect outreaching, as they suppose, all medical skill; they usually therefore solicit the supernatural aid of charms through persons called "fairy doctors." I, however, have had in the course of years, sufficient experience to give the limited attestation of digitalis above described.

Convinced that nothing has been created in vain, and having heard, I believe truly, of the disappearance of white swellings in some young patients, and witnessed the cure of chronic and ill-conditioned eruptions, solely by the power of herbs administered by old women in Ireland; besides the removal of that inflammatory disease called farcy in horses, not unfrequently by one dose after bleeding, often, after an interval of three days, by two, and in inveterate cases, by three doses, of a decoction of roots and herbs, with the addition of a little salt; I am of opinion that the botanical kingdom is too much neglected by the physicians and surgeons of Great Britain and Ireland, and that a mean ought to be observed in the treatment of some diseases, between the partiality for herbs on the Continent, and that for chemical preparations among British medical practitioners.

CASE OF

MALIGNANT CHOLERA,

FATAL IN TWELVE HOURS, THE PATIENT PREVIOUSLY ROBUST.

To the Editor of THE LANCET.

SIR,—A second case of malignant cholera has occurred at the workhouse of this parish, which was fatal in twelve hours and a half. I will not occupy your valuable pages by entering into any minute detail of symptoms or treatment, as it will be sufficient for every practical purpose to give a brief outline of them. I am, Sir, your obedient servant,

JOHN BIRTWHISTLE.

Maida Place, Mile End Road,
March 20th, 1832.

CASE.

Jane Alexander, ætat. 10, described by the matron as a florid, full-fleshed, and beautiful child, who had enjoyed most excellent health up to the time of attack, with the exception of occasionally suffering a little from indigestion, was seized on Saturday last at eight A.M. with symptoms of cholera. She had no previous diarrhœa, but experienced a fit of vomiting on the preceding evening, which continued however only a short time, and no notice was taken of it. She was not seen by the parish surgeons until eleven, as they were not earlier apprised of her illness, when she was found in such a state of collapse that no hopes were entertained of her recovery. There was this peculiarity in this case, that the spasms of the extremities, although exceedingly severe, did not come on so frequently as in the first, and the same remark will apply to the purging and vomiting. Indeed for some time before death, the patient appeared not to have sufficient power to eject what was given her, the prostration of strength was so great. The evacuations had the decided rice-water character. The urine was completely suppressed after ten A.M., at which time she passed a little, which had been no doubt secreted before the disease came on. The pulse from the commencement might be said to be imperceptible at the wrist, and could only be indistinctly felt in the carotids. The usual means of heat, frictions, opiates, and stimulants, were had recourse to, and had the effect of producing at one time, it was thought, a slight effort to rally, when a vein was opened in the arm, but the blood could only be obtained in drops. By perseverance, however, three ounces were obtained in this way, but without giving any relief. She gradually became worse, and died at half past eight in the evening.

The post-mortem examination took place at one P.M. on the following day, and was attended by Messrs. Caruthers, Snow, Dowers, and several other medical gentlemen. The omentum and small intestines presented a slight roseate appearance. The mesenteric veins were generally, but not greatly, congested, and the glands were a little enlarged. From the termination of the ascending colon to the anus, the large intestine contained a quantity of white, clear, shining, inodorous fluid, of the consistence of thick cream, and which bore a very near resemblance to well-boiled rice-water gruel. None of the medical gentlemen present had ever seen a fluid of a similar nature, except in this disease. The bladder was contracted and empty. The gall-bladder was filled with bile, but none was observed in the intestines. The brain was

examined and found to be slightly congested. The other viscera were healthy.

It has been attempted by some, to prove that this is a disease of starvation, but this case utterly refutes such a doctrine. The girl, although an inmate of the workhouse, was well-clothed and fed, and was, as I have already stated, in excellent condition prior to being seized. It cannot be denied that the dreadful state of destitution and distress in which the poor of this metropolis are at the present moment, greatly predisposes them to the disease; but it should at the same time be known, that others in very different circumstances are sometimes attacked by it.

Before I conclude these remarks, I would wish to observe, that I do not consider suppression of urine an invariable symptom in this disease, for in one case that I attended in Bombay, water was voided in some quantity two hours before death.

ATTEMPT TO EXPLAIN THE

PHYSIOLOGY OF THE MALIGNANT CHOLERA,

WITH DEDUCTIONS INDICATING

ITS TREATMENT.

To the Editor of THE LANCET.

SIR,—Without meaning to deny that all the phenomena which distinguish a case of spasmodic cholera may be referable to the sudden action of a violent poison on the nervous system, I think that most of them admit of explanation, from the single fact brought to light by the analysis of cholera blood, viz. the loss it has sustained in its watery constituents.

In health the kidneys are the organs by which the superfluous serum, with its redundant salts, are carried out of the system; but in cholera this office appears to be taken up by the mucous membrane of the stomach and intestines. Under this usurpation the function of the kidneys ceases of course, and nature speedily sets about the process of abolition in the bladder, as a viscus no longer necessary. This I believe to be the physiological explanation of the total suppression of urine, and the contraction of the bladder, which are the invariable accompaniments of this disease.

When abnormal function is once set up in any organ, it is always inordinate in degree. Thus what was *well done* by the kidneys, becomes *excessive* from the mucous surfaces. The red globules, at any rate, and perhaps the fibrine of the blood, remain in excess—not in proportion to the healthy demand of the system, but in relation to itself. Thus its physical condition is changed, and particles which were retained at proper distances by the fluid in which they floated, are necessarily brought into closer apposition by its abstraction, thus making it as unfit for, as its diminished fluidity renders it difficult of, circulation.

But the arteries contract upon their contents, and readily adapt themselves to the quantity, whenever increase or diminution is not too sudden. In cholera they appear to perform this action spasmodically, and actually to squeeze out the more fluid part of their contents through those textures of the body which present the least resistance. This close contraction upon the colouring particles of the blood may account for *the blue tinge*, which, coupled with the profuse and sudden loss of the fluid contents of the blood-vessels, amply explains the collapsed and shrivelled appearance of the sufferers.

This view leads to some indication of a proper mode of treatment. I conceive that our first attempt should be directed to the altered state of the physical condition of the blood. We know that water, in the *healthy* condition of the body, very speedily finds its way to the blood-vessels; it is the only liquid which can so enter the circulating system *in large quantity*, and with the same rapidity, without undergoing the process of digestion. Doubtless it does so enter by imbibition from the mucous surface of the stomach. Call it Endosmose if you will; but the fact remains the same. It is highly probable that all mucous surfaces possess, more or less, the same power of imbibing watery fluids which are not uncongenial to the blood; and that they exercise it in common with the stomach, up to the point of saturation. Fever, too, presents an example where the same process goes on with uninterrupted activity in a *diseased* condition of the body. I would apply this fact to the treatment of cholera. Instead of ardent spirits and powerful stimulants, which, in lieu of additional fluidity, can only supply destructive principles, I propose that pure distilled water be liberally poured into the stomach, and also into the rectum; and that the volume of remaining blood be somewhat diminished, if possible, by bleeding, or at any rate by cupping and leeching, by way of approximating its existing proportions to the aqueous fluid now introduced.

The feelings of the patient would lead me to indulge him with the water quite cold; for nothing is so grateful, or so much craved, as pure cold water: that which is thrown into the rectum should approach the natural heat of the body. I do not propose to deluge the stomach at once with an overpowering quantity of liquid, but as soon as possible after the supervention of collapse

(for to that state alone do my remarks apply), I would give four ounces of distilled water, or of soda water if vomiting was present, and would repeat this every ten minutes, for an hour or longer, according to circumstances. I need say no more on this point: the hint, if approved, will be enough. If the principle be good, the practice may be modified, but not condemned.

In the after treatment calomel follows of course, as the only means of speedily breaking down the inspissated texture of the blood, which makes instantaneous dilution so essential.

If counter irritation should be thought advisable, it may be speedily and easily effected without much suffering to the patient, by dabbing the scrobiculus cordis with rectified spirit, and setting the liquid on fire for a few seconds. I forbear to trespass further on your valuable space, by extending my remarks beyond that hopeless stage in which medical treatment in this country has hitherto proved so unsuccessful. The plan proposed is, to me at least, novel: and has both simplicity to recommend it, and something like reason to sanction its trial. Its adoption also will prove grateful to the feelings of the patient, by slaking his burning thirst; at the same time it presents nothing to alarm or shock the prejudices of his attendant friends,—a matter which I can assure you is, in *this* part of the kingdom and *at the present moment*, of no mean importance.

I remain, Sir,
Your very obedient servant,
ALFRED HARDWICK.

175, Buchanan-street, Glasgow,
11th March, 1832.

P.S. TEMPERATURE OF CHOLERA PATIENTS.—There are some curious facts not yet generally known connected with the temperature of individuals in the collapse stage of cholera. As I believe these will shortly be made public by another person, I will only now say, that the heat varies in different parts of the body, being, for instance, several degrees higher in the axilla than in the mouth, though both are far below the proper standard. But the most curious fact is, that some hours after death, the body, which has during life effectually resisted external heat, however assiduously applied, resumes its natural living standard, having a GENERAL *warmth* diffused over it, although removed to an apartment below 50° Fahrenheit, and exposed to the air. A gentleman whose attention I particularly directed to this point, found the temperature in one case 104°, two hours after apparent death, and under circumstances of similar exposure. I do not feel myself at liberty to say more on these points at present.

LONDON PHRENOLOGICAL SOCIETY.

January 30, 1832.

Dr. ELLIOTSON, President, in the Chair.

MR. NOEL was elected an ordinary member. Mr. Colpitts Grainger was proposed as an ordinary member. A paper was then read by Mr. Weisten, entitled

SOME REMARKS ON DREAMING, SOMNAMBULISM, AND OTHER STATES OF PARTIAL ACTIVITY OF THE CEREBRAL FACULTIES.

The functions of a living being are divided by physiologists into those of vegetative and animal life. The vegetative life, both in brutes and in man, is in every respect analogous to the life of plants. All its functions are performed without consciousness, and involuntarily. From the first moment of the existence of a being, until its death, the organs of the vegetative life act uninterruptedly with greater or less energy. The functions of respiration, digestion, secretion, circulation, &c., are of this class.

All the functions of the animal life, on the contrary, are performed with consciousness; and the brute or the man has the power of acting voluntarily on the organs of those functions. When they are fatigued or exhausted by exercise, they require intervals of repose, in order to resume the strength necessary to enable them to continue their actions.

The functions of animal life are in fact those of the brain. During sleep the brain alone is in a state of rest, all the other organs performing at that time their usual functions; it is wrong, therefore, to say the body rests in sleep, for we should then, as is too frequently done, mistake the cerebral functions for those of the whole organic system.

After a day passed in the active and healthy exercise of the animal faculties, the irritability of the brain diminishes, the external and internal sensations become duller, the mind becomes incapable of continued attention, of combining ideas, or of exercising the power of volition over the muscular contractions; the limbs support with difficulty the weight of the body, which assumes a posture requiring the least muscular effort to sustain it; the speech becomes slow and unconnected, the eyelids fall, the muscles of the back of the neck cease to act, and the head falls forwards; in this state the mental sensations and images become more and more faint; and by insensible degrees all consciousness and volition are lost; in other words, the cerebral

functions are completely suspended. The individual is then said to be in a deep sleep.

Cabanis has emitted the opinion, that "sleep is not simply a passive state, but a peculiar function of the brain." This proposition is obviously false, the cessation of a function cannot itself be a function.

It is well known, that during sleep the temperature of the body is diminished, and there are few persons who have not experienced during the night, on waking suddenly, a general coldness, which, a few minutes after the re-establishment of the cerebral functions, has been converted into a gentle warmth, diffusing itself over the body, but especially to the feet.

A very important fact, and to the establishment of which a great number of observations concur, is, that during sleep contagious miasma of all kinds exercise their pernicious action on the system with increased energy; and if we compare this fact with another also well known, viz. that the activity of certain moral faculties, such as courage, confidence, firmness, &c., enables the brain to resist these same causes, we are led to conclude, that the activity and energy of the brain, and the proper direction of its functions, are so many circumstances which add to the force of resistance of this organ, whilst on the contrary, sleep, and the depression of the moral faculties, tend to diminish this force of resistance.

Much controversy has resulted from the question, whether the soul can ever exist, for instance, in sleep, without experiencing any sensation or emotion, or without having any idea. If we cast aside all vain metaphysical subtleties, the solution of this question, as Dr. Gall observes, is extremely easy. In this life the soul receives its emotions and its ideas through the means of the cerebral organs; when, therefore, these organs are completely inactive, the soul can have neither emotions nor ideas. A deep and perfect sleep, is a temporary cessation of all the functions of animal life, viz., of thought, sensation, mental emotion, volition, and all their combinations and modifications.

DREAMING.

In perfect sleep, as I have before stated, all the cerebral faculties are suspended; when however (the power of volition being suspended, and the mind inattentive to the suggestions of the sensations) certain of the perceptive faculties and the feelings remain in a state of activity, or become so, their manifestations take place independently of the will; and this state of the mind is called dreaming.

Sometimes the perceptive faculties alone are awakened, and these objects, scenes, and circumstances, present themselves to the mind, unassociated with those feelings with which they are usually or invariably accompanied in our waking hours. Thus in our dreams we may walk on the brink of a precipice, or see ourselves doomed to immediate destruction by the weapon of a foe, or the fury of a tempestuous sea, and yet feel not the slightest emotion of fear, though during the perfect activity of the brain, we may be naturally disposed to the strong manifestation of this feeling; again, we may see the most extraordinary object or event without surprise, perform the most ruthless crime without compunction, and see what in our waking hours would cause us unmitigated grief, without the smallest feeling of sorrow. Dreams of this kind are more incoherent, and are subject to more rapid transitions, than those in which one or more of the organs of the feelings are also in a state of activity.

When the organ of any feeling, or several of them, are in a state of activity, the dream takes its form from these awakened feelings, and the perceptive faculties furnish images in correspondence with them. In such cases objects and events are represented, which excite in us wonder, rage, dread, love, pity, &c. The more organs there are in simultaneous activity, the more complicated will be the action which the dream represents; and its rationality or irrationality will depend on the correspondence or want of correspondence between the mental perceptions and the normal state of the intellectual and affective faculties.

Several philosophers have asserted, that our dreams consist of nothing but the repetition of ideas which have already passed through the mind, though they may exist under new combinations. This, as Gall properly observes, is an error. Man may exercise the faculties of invention during his sleep, as in his waking hours; for the internal sources of his thoughts and feelings are the same, whether he sleeps or wakes. Cabanis relates of the illustrious Dr. Franklin, that he had on several occasions in his dreams been informed of the issue of affairs in which he was engaged at those times. His vigorous mind, otherwise entirely free from prejudice, says this author, could not quite guarantee him from a superstitious notion with respect to these premonitions. He did not take into consideration that his profound prudence and his rare sagacity, still directed the action of his brain during sleep. It is also related of Condillac, that whilst writing his Cours d' Etudes, he was frequently obliged to leave a chapter already prepared but incomplete, in order to sleep, and that on awaking he found it, on more than one occasion, finished in his head. Vol-

taire relates, that on several occasions he made verses in his sleep which he remembered when awake, and the same thing is told of Augustus Lafontaine. Alexander formed the plan of a battle while in the same state; and Tartini, a celebrated performer on the violin, composed his Devil's Sonata from the inspiration of a dream, in which his satanic majesty appeared, and challenged him to a competition on his own fiddle. These facts abundantly prove, that all the powers of the imagination and intellect may be active in dreams. Yet I will venture to quote another remarkable instance, in the case of a distinguished living poet, Mr. Coleridge. In his works, a poetical fragment, entitled Kubla Khan, is prefaced as follows:—

"In the summer of the year 1797, the author, then in ill health, had retired to a lonely farm-house between Porlock and Linton, on the Exmoor confines of Somerset and Devonshire. In consequence of a slight indisposition, an anodyne had been prescribed, from the effects of which he fell asleep in his chair at the moment that he was reading the following sentence, or words of the same substance, in Purchas's Pilgrimage:—'Here the Khan Kubla commanded a palace to be built, and a stately garden thereunto; and thus ten miles of fertile ground were enclosed with a wall.' The author continued for about three hours in a profound sleep, at least of the external senses, during which time he has the most vivid confidence that he could not have composed less than from two to three hundred lines; if that indeed can be called composition in which all the images rose up before him as *things*, with a parallel production of the correspondent expressions, without any sensation, or consciousness of effort. On awaking he appeared to himself to have a distinct recollection of the whole, and taking his pen, ink, and paper, instantly and eagerly wrote down the lines that are here preserved. At this moment he was unfortunately called out by a person on business from Porlock, and detained by him above an hour, and on his return to his room, found, to his no small surprise and mortification, that though he still retained some vague and dim recollection of the general purport of the vision, yet, with the exception of some eight or ten scattered lines and images, all the rest had passed away like the images on the surface of a stream into which a stone had been cast, but, alas! without the after restoration of the latter."

On the table there is a bust of this talented poet; and on referring to the extraordinary development of the organs of the perceptive faculties of form, locality, language, &c. there manifested, it will not create much surprise that faculties so unusually predominant should be subject to the degree of excitement above described.

Dreams have served as a text to many metaphysical discussions, and given rise to many superstitious beliefs. Some have seen in this phenomenon a most certain proof of the existence of two natures; for, say they, sleep is the repose of the body, and the soul is then insulated, is liberated from its terrestrial bonds, and thinks by itself; if this be indeed the fact, the soul thinks very oddly when it thinks by itself. Others have considered dreams as something supernatural, nor is it wonderful that their suggestions should be sometimes prophetic; it is nothing strange that the mind foreseeing the probable course of events, should represent their result in a dream, nor even that the fortuitous combinations of the dreaming fancy should be sometimes realised.

It has been remarked that, ordinarily, dreams do not occur during the first hours of sleep, unless the brain be extremely irritable. But in proportion as the organs recover from this fatigue, they are more disposed to enter into activity, and this explains why, as we approach the hour of waking, our dreams are more frequent and vivid. Dreaming is therefore only a partial awakening, an involuntary activity of the cerebral organs whilst others repose, and the different phenomena it exhibits give an additional proof to the numerous ones we already possess of the plurality of the intellectual and moral faculties.

In all the phenomena of dreaming to which I have hitherto alluded, the dreams have arisen from the spontaneous activity of some of the perceptive, intellectual, and affective faculties, unconnected with the suggestions of any of the external sensations. But the mind may be in such a state as to be attentive to these sensations, and then the accessory images created by the imagination unite with them, and form a dream of a still more vivid nature than any before noticed.

If you place near a sleeping person a rose, so that its perfume may affect his sense of smell, this sensation, by the association of ideas established in his waking hours by the simultaneousness of the impressions, may give rise in his imagination to the visual image of a rose, or to that of a garden of roses. An invalid attacked by the gout, and beginning to feel pain in his joints, may dream that he is undergoing the torture, and may see the executioners tear the part at which he suffers. Some experiments made by M. Giron de Buzareingues, with the intention of ascertaining whether a person may determine the nature of his dreams, are recorded in the eighth volume of Magendie's journal.

In a first experiment, having allowed the posterior part of his head to remain uncovered during sleep, he thought he was at a religious ceremony performed in the open air; the custom in the country in which he dwelt, and to which he conformed, was to have the head constantly covered, excepting in some rare circumstances, among which were specially reckoned these ceremonies. He felt, on waking, cold at the back of the neck, as he had frequently felt in the real scenes, the representation of which had just presented itself to his imagination.

He repeated this experiment, allowing several days to elapse between the two, in order to convince himself that the first result was not the effect of chance; the second vision was almost entirely similar to the first.

In a third experiment he left his knees uncovered, and he dreamt that he travelled during the night in a diligence. Now all travellers, he observes, know that in a coach it is principally at the knees that cold is felt during the night. He adds that none of these dreams referred in any ways to the occupations of the preceding days.

With dreams of this kind must be classed those imperfect slumbers, after which, persons, awaking, have answered to observations made during their repose; this in a great measure resembles the state of mind called abstraction, in which there appears to be a passive consciousness of that which, because the feelings which are usually associated with it are inactive, does not the least disturb the internal meditation.

The phenomena of nightmare differ from those of ordinary dreaming, particularly in this circumstance, that whilst the dreamer is incapable of exercising the power of volition, he is distinctly conscious of his inability to do so; another circumstance which distinguishes this state of the cerebral functions is, that it is always accompanied by aggravated feelings of dread and terror. Perhaps the over-excitation of that organ, of which fear is a manifestation, is the predisposing cause of this distressing malady. Several physiologists have attempted to give mechanical explanations of this phenomenon. It has been affirmed that its cause is the compression of the vena cava by the stomach, and it has been attributed to other causes as little probable. But that the nightmare is solely a cerebral state, the following facts are sufficient to prove:—1st. That the phenomena which characterise it are cerebral, and disappear immediately after waking, without any trace remaining of the oppressive suffocation which was experienced the preceding instant; 2d. That it never occurs in the waking state, whatever position be assumed, or whatever be the state of the body, and that it most frequently occurs when the brain is irritated or affected.

When one or more of the powers of voluntary motion are in activity, simultaneously with some of the intellectual and affective faculties, those states called somniloquism and somnambulism result. The most ordinary cases are those in which the external senses are inactive; the sleeper then talks himself, but without understanding what is addressed to him; or he gets up, walks about, opens doors and windows, &c. In other cases some of the external senses are also active; and by paying proper attention to the different combinations or the organs of the intellectual and affective faculties with those of the senses, and also those of voluntary action, we may easily be made to understand the astonishing phenomena which this extraordinary state of mind presents.

A few cases quoted from different authors will exemplify some of the varied forms under which somnambulism occasionally appears:—

Dr. Gall relates that he saw at Berlin a young man, sixteen years of age, who had from time to time very extraordinary fits. He moved about in his bed without consciousness; his motions and his gestures manifested a great activity of several of his internal organs; whatever was done to him he perceived nothing; at last, he jumped out of bed, and walked with quick steps about the room: his eyes were then open and fixed. Dr. Gall placed several obstacles in his way, he either removed them with his hand, or carefully avoided them; he then threw himself suddenly again on the bed, moved about for some time, and finished by jumping up awake, much astonished with the number of curious people who surrounded him.

In the Encyclopædia Methodique is an account of a young student who got up to compose a sermon whilst asleep, wrote it correctly, read it over from one end to the other, or at least appeared to read it, made corrections in it, scratched out lines and substituted others, put in its place a word which had been omitted; composed music, wrote it accurately down, and performed other things equally surprising. This is related on the testimony of an archbishop of Bourdeaux, who was witness to the facts.

Dr. Gall relates several curious cases, one of a scholar finding out places in a geographical map when asleep; another of a miller who was in the habit of getting up every night, and attending to his usual avocations at the mill, then returning to bed, and on waking in the morning remembering nothing of what passed during the night.

The same thing is observed in somnam-

bulism as in ordinary dreaming, as regards the want of association between the acts in one case, or the imagination of them in the other, and the feelings which usually or invariably accompany these acts in the perfect waking state of all the cerebral faculties. Thus a somnambulist will walk along a narrow parapet, at a height from which were he to fall he could not escape with life, and yet he will have no more fear than if it were only a few inches from the ground; this is because the organ which manifests that feeling is asleep in him. If you now awake him you restore this feeling, and he is inevitably lost; not because he is then incapable of walking on the parapet, but because fear has taken from him all confidence in his powers.

I will now conclude with the observation that all the phenomena of dreaming and somnambulism prove the truth of this proposition:—that certain organs or even certain senses may be separately in activity, whilst others are completely inactive or asleep.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE IN THE UNIVERSITY OF LONDON,

Delivered Feb. 20th, 1832.

CASE OF DISEASED LUNGS.

Cause of Crepitous Rattle.—Pectoriloquy.

The parts before me, Gentlemen, were taken from a man who was admitted on the 9th of February into William's Ward. He was thirty-nine years of age, and complained of difficulty of breathing, with a troublesome cough. He was a stout athletic man, with remarkably fine muscles. The greater part of his muscular system was really so fully developed, that he might have very well stood before a sculptor as a model.

He said that he had been ill only six weeks, but it is quite certain that he must have been ill much longer than that. The accounts which patients give you at a hospital are very deceptive. They tell you that they have been ill only a week, when in reality they have been ill a twelvemonth. The fact is, they generally calculate from the time they were unable to work, or from the time that they found themselves particularly ill. Their extreme illness makes them forget their original indisposition. This patient's pulse was quick. He had been confined to his bed, and was continually coughing, especially on inspiration, but had had no pain that I recollect. On listening to his chest there were sonorous and sibilous rattle showing bronchitis, and there was likewise a crepitous rattle, showing inflammation of the air-cells; that, at least, I presumed,—from the circumstance that he declared he had been ill only six weeks.

The crepitous rattle will arise, I believe, under three circumstances;—The one is, when there is hæmorrhage into the air-cells, in which case we have generally a very copious discharge of blood; when the blood comes merely from the bronchial tubes, the hæmorrhage is comparatively slight; but when it proceeds from the air-cells, there is generally profuse hæmorrhage, and frequent attacks of it. By far the more frequent attacks of hæmoptysis come from the air-tubes. In this case there was no blood spit up, and there was no want of respiration in any one part of the chest, nor any dulness of sound, as is usually the case when there is hæmorrhage from the air-cells. When there is hæmorrhage from the air-cells, we have usually not only a crepitous rattle, but from the blood being effused, the lungs become much harder at one part, so that frequently to some extent we have no respiration at the spot, and immediately around it you have crepitous rattle.

Now in this case there was nothing of the kind, and therefore I presumed it was not crepitous rattle from a discharge of blood into the air-cells of the lungs.

Another cause of crepitous rattle is long-continued bronchitis, through which the air-cells become very much dilated, and perhaps ruptured. In this case they are in the state of those of a broken-winded horse, I believe. I am told that a rupture of the air-cells in a horse, or their excessive dilatation (and I believe that where there is a dilatation, many are often thrown one into another), is a common cause of that animal being broken-winded. Whether this is correct or not, I cannot pretend to say, not understanding the subject. But in the human being, where bronchitis has continued for a long time, you may find the air-cells very much dilated, and frequently they are thrown one into another. However, as this man had been ill only six weeks, I could not ascribe the crepitous rattle to that.

Another circumstance under which crepitous rattle takes place particularly, is inflammation of the air-cells; that is to say, peripneumony,—inflammation, as it is called, of the substance of the lungs. When these are inflamed, a very tenacious mucus is secreted in them, excessively tenacious, and you have a crackling sound. Now there was a crackling sound all over this

man's lungs. Before exhibiting the specimen, I will show you a representation in Dr. Baillie's work on Morbid Anatomy of dilated air-cells, for although the French have described their dilatation very well, Dr. Baillie had noticed them previously. Here is a representation of dilated air-cells. We have several very fine specimens of it in the museum. We have one specimen where the air-cells are dilated to the size of a walnut. A large cavity exists in the surface of the lungs, a bladder, as it were, as large as a walnut.

From the quickness of the pulse, the heat of the skin, the difficulty of respiration, frequent cough, and discharge of mucus, it was clear that this man was labouring under an attack of inflammation of the lungs, either bronchitis or peripneumony, —inflammation of the air-tubes, or inflammation of the air-cells; but as I heard crepitous rattle in addition to sonorous and sibilous rattle, I concluded that he had both affections. In bronchitis there is sonorous rattle at the part where the inflamed bronchiæ are situated, and where there is peripneumony there is crepitous rattle. This man had both. Respiration was morbid all over the chest. There was no sign of consolidation of the lung in any part, nor any sign of effusion into the chest, and no acute pain showing pleuritis.

Treatment.—The man was bled to a pint. The blood was much buffed and cupped, and he had half an ounce of antimonial wine every eight hours, was put on slops, and on account of the cough he had ten grains of extract of conium every night. On the 13th it was necessary to bleed him again, and the grain of tartar emetic contained in the half ounce of antimonial wine was given every four hours. On the 14th it was necessary to bleed him again, and as the antimonial did not appear to be doing any good, I changed it for what I know is better, calomel, but which I at first thought might not be required in this case, and of which he took three grains every four hours. He had also a large blister on the chest. It was found necessary on the 15th to cup him between the shoulders to fifteen ounces. Another blister was applied, and the calomel was given every four hours, but there was no remission in consequence of this treatment, and he died.

Post-mortem Appearances.—It was very clear, on opening him, that the man, although he had suffered an acute attack which had aggravated all the symptoms, had been labouring under the disease for many months—for what I know, twelve months. It was no wonder, therefore, that this free bleeding, the exhibition of antimonials, of calomel, and the application of the blister, did him no good.

Now the crepitous rattle arose—I will not say altogether, but in a great measure —from dilated air-cells. At the time we opened the body, we found a considerable number dilated. It was decidedly evident that he had long been the subject of chronic bronchitis. I am afraid that this appearance of the air-cells is now all gone. However, we found a considerable number in a state of dilation, and some probably in a state of rupture. Besides this, however, there was found a very large cavity on the summit of one lung. Now the existence of this cavity—a phthisical cavity no doubt —I did not know during his life. It is true I began to suspect that he had been ill a much longer time than he had stated, and I mentioned to gentlemen at the bed-side that I considered it likely that he might have incipient phthisis, that underneath his other disease we might find tubercles in the lungs, but I said no more upon the subject. It is very common among patients, when they come with bronchitis, to see enlarged ends of the fingers; and though you may diminish the other symptoms, yet the patient may labour under phthisis, and phthisis soon manifests itself. They probably have tubercles at the time, which are now in a state of progression. I thought it likely in this case, but I said nothing more about it. I examined him carefully, and there was no pectoriloquy, and that is a point on which I am anxious to dwell, because here is a large cavity in the upper part of the left lung.

Now you will of course inquire how it was that there was no pectoriloquy. Was I deceived, or could such a cavity exist without pectoriloquy? It is allowed by all those who understand the subject, that you may have a large cavity in the lung without pectoriloquy. When a cavity is only of a certain size, and bears a certain proportion to the bronchial tubes, you have pectoriloquy, and when you hear pectoriloquy in the lungs or the sound of the voice going through the stethoscope into the ear, you can have no doubt about it, and you may depend on it there is a cavity. You cannot tell by these means what is the nature of the cavity. It may be from a common abscess, it may be from a scrofulous phthisical tubercle; it may be very dilated tubes; it may be a cavity from a portion of the lung having become gangrenous, sloughing off, and discharged. You cannot tell what is the nature of the cavity; you can only tell that there is a cavity. Whenever you discover pectoriloquy, you may be sure there is a cavity It is only in old cases that the bronchiæ are dilated, and then there is an absence of the signs of phthisis in general, and the patient has long been subject to chronic bronchitis.

But although when that is the case, you may infer that there is a cavity in the substance of the lung, or a bronchial tube or tubes so dilated as to form what is tantamount to a cavity, yet it is to be remembered, that you may have phthisical tumours in different stages without pectoriloquy. At the beginning of phthisis, tubercles may exist without suppuration, and therefore without a cavity, and the stethoscope will not tell you their existence. If, indeed, the tubercles be clustered together in a mass, you find at that part a dull sound on percussion, showing that the lungs are not pervious there; and on listening, you find no respiration there, or at least very little; but if they be not in a mass before they suppurate, you must depend in your diagnosis upon the general signs,—if on observing a patient he looks pale, is extremely delicate, his upper lip is swollen, his teeth are very white, there is a transparency of his eyes which cannot be mistaken, though so strong, is it indicative of phthisis, and, as I mentioned before, if the ends of the fingers are enlarged, and, above all, if there is a constantly quick pulse, a pulse easily excited, stitch in the side, short cough, and perhaps bloody expectoration,—these things will show clearly enough that a person is labouring under phthisis, or is on the brink of that affection.

Now, not only at the beginning, but also at the end of phthisis, the stethoscope may be of but little or no use. For, if the cavity which exists in the lungs become very large, so as to bear an excessive proportion to the bronchial tubes,—if the bronchial tubes are quite small compared with the cavity at large, you have no pectoriloquy, more especially if the walls be thin. It is necessary to pectoriloquy that the walls should be of a certain thickness; for if they be not thick, or rather thick, you will not have that sound. Now in general there can be doubt in these cases, notwithstanding the cavity does not give pectoriloquy, for, in the first place, you have seen that it is possible to state that the patient's case is probably one of phthisis without the use of the ear at all. In some cases you may have heard pectoriloquy at a particular spot, and then it will cease, and if the cavity do not maintain a relative size to the opening of the bronchial tubes, then you have no pectoriloquy, and therefore at last you find no pectoriloquy, although you may have observed it previously. You may have heard pectoriloquy at one spot previously, so as to be satisfied in your mind about it at that time; but the cavity may have ultimately become so large, that it will not give pectoriloquy; but in these cases you most likely have cavities in other parts of the lungs which do.

Now you see in this case those two circumstances existing which appear to prevent pectoriloquy. The cavity, in the first place, is very large,—out of proportion to the bronchial tubes that open into it. In the second place, the walls are very thin; there is scarcely any substance between the cavity and the surface. They are as thin as they well can be. Here is the pleura. There is scarcely any pulmonary substance; in fact, the wall of the cavity is transparent; the lungs are completely gone here. Now to prove that there is a very old cavity, you have only to feel it, and on doing so you find that it is exceedingly firm—almost cartilaginous. I cannot make any impression on it, even with my fingers. Now when cavities of this kind are old, the walls acquire a cartilaginous hardness, and the inner orifice is smooth, and the wall next the cavity of the pleura is thin. In the next place, in old cavities the cavity is complete. In new cavities of a moderate size,—not very large, but which are getting larger,—which have not existed for a long time, you see bands of pulmonary substance going across the cavity, traversing it in various directions, with tubercular matter on them. They are composed of bronchiæ and a quantity of pulmonary substance. There is an additional proof of (I was going to say) its antiquity. Speaking of its walls you observe that it contains bone. There were several particles of bony substance, together with cartilage, which would not have been the case unless the cavity had existed for some time. It is singular, but on making a section there is hardly any other part of the lung with tubercular deposition. This man must have been ill a very long time with cough, and with expectoration. There are, as you observe a few other tubercles in the section I have now made, but they are of very recent formation. When you consider that the air-cells were ruptured or dilated in different parts of the lungs, there can be no doubt whatever, that not only the cavity has existed for a length of time, but the patient must have had bronchitis for an equally long period,—perhaps for years.

This is very instructive, as showing how much reliance is to be placed on the ear; because some persons have certainly supposed that everything is to be done in diseases of the lungs by the ear; but nothing can be more unphilosophical. Nature has given us five senses, and it is necessary to employ any one of them that can be an avenue of information. It is as absurd to depend on the ear alone in diseases of the chest, as it would be to depend on the eye alone. In all these diseases it is necessary in the first place to attend to the aspect of the patient, to look at his countenance, to

take into consideration all his symptoms, to get from him a history of his case, and examine the state of his pulse. So that here you have the use of the eye and of the sense of touch; and, in addition to these, it is likewise of the highest importance to employ the aid furnished by the ear. At the beginning the ear will serve you but little. You must then depend chiefly upon the aspect of the patient, and those symptoms to be observed by the eye, by the touch, by listening to his narrative or by reading it. Then, after a time, the ear will corroborate your opinion. In general, unfortunately, in phthisis, it will soon become too certain that the patient is labouring under a mortal disease; but sometimes it is of great use to prove this. Now and then you meet with medical men who are full of dyspepsia, who talk of nothing but chylopoietic viscera, and call everything dyspepsia. In such a case it may be useful, when you are really consulted on a case of phthisis, to prove to your colleagues and to others who are present, that the disease is one of decided phthisis,—to prove by the ear that there is a cavity in the lungs. Sometimes you will meet with men who so pervert their judgments, that they will not allow there is phthisis, and sometimes a family will not believe that one of its members can be labouring under that complaint, and they will tell you that you cannot see into a patient's inside, and consider that you cannot tell the exact state of the lungs. But if by means of the ear you can ascertain the fact, it is of very great importance. Unfortunately in phthisis, the ear is often too decisive, but sometimes it is of great use when you cannot get a history from the patient (and patients among the poorer classes of the people are often so stupid, that they cannot give you satisfactory information), to apply the ear to the chest, by which means you ascertain the exact nature of the affection. Sometimes, too, a patient is labouring under another disease,—bronchitis, and you may not be able to tell without the ear that he has another affection. Now in this individual, if the cavity had not been so large, if he had come to the hospital perhaps four or five months ago, the cavity would then have given signs of pectoriloquy, and one could at once have said that there was phthisis as well as bronchitis. Therefore it is of use in complicated cases to gain what information the patient will impart. On the other hand, however, you see that there are cases in which the ear will not tell everything. Here was a large cavity without giving any sign, at least any which my ear could detect, or which led me to suppose its existence, and you see the reason of it. You perceive that a cavity of this size may exist, and yet give no signs of its presence.

Now it is just the same in diseases of the heart; the stethoscope is of the highest use, it will save a great deal of trouble; but on the other hand there are a variety of symptoms which display themselves to the eye and to the touch, and a great deal is to be learned from the history of the case given by the patient, all of which may show the nature of the case without the aid of the ear. And again, there are organic diseases of the heart which will give no signs whatever. You may have ossification of the coronary arteries without any signs being afforded by the ear, and after the most intense disease of the heart, near its close, you may not discover it by the stethoscope. I had a striking instance of this only yesterday. I was consulted on the case of a gentleman about seven or eight months since, who had had dyspepsia all his life, but at this period he was labouring under a very severe attack. He had great flatulency and acidity; he was extremely weak, and his legs were swelling a little, but he would not allow that there was anything the matter with him except his dyspepsia. I made a very careful examination of his case, I examined the chest, and found it clear that he laboured under an enlargement of the heart; that there was a strong action of the left ventricle over a larger space than in health. It was clearly a case of dilatation of the left ventricle with a thickening of the walls, or at least that there was no diminution of them, which is tantamount with dilatation to a thickening. I did not tell him so, but I deemed it my duty to inform his wife of it, and requested her to make up her mind to lose him before long; that perhaps he might survive a year or two, but he might die considerably sooner than that, which would depend upon accidental circumstances. He was treated upon the plan which I have frequently recommended in these cases, and became so much better that he considered himself nearly well. He took digitalis, squills, acetate of potass, and nitrous ether. The action of the heart subsided very much, all the signs of effusion ceased, and he was as well as he had been for years. About a week ago, he ate something which he ought not, and he ate more than he should, and in consequence of his extreme indisposition I was sent for. I found his pulse varying from 140 to 160, and exceedingly feeble, scarcely to be felt; in fact I could not count it at the wrist, and was obliged to do so at the heart. There were constant eructations through having thus disturbed his stomach, and it appeared to me that most likely his stomach had become soft.

I have seen the stomach become soft very rapidly; I have seen the liver become soft in four or five days, and the patient sink. Having been suspected to be wrong former-

ly in stating that he had diseased heart, I now examined that organ from curiosity, and I found no sign of diseased heart, though I was satisfied before that there were both hypertrophy and dilatation. His heart was beating so feebly, that it gave no sign of disease at all, and he was just in that state that we see persons when they are sinking under a particular disease. He had a vermicular pulse, it was more like the moving of a worm, than anything else. Such extreme debility as this could not be recovered from, and he died without any signs of disease of the heart. I opened him between five and six o'clock last evening, and there was the heart as big again as it should be, with the cavity of the left ventricle three times its natural size, and as it maintained its original thickness, there was of course a great addition of substance. Here was an exceedingly diseased heart; but had I not been called in before, and only seen him at my last visit, about eight days before his death, I should not have discovered that there was disease of that organ. The power of the patient's system was completely gone, so that this hypertrophied heart was no longer in a condition to give signs of disease, to give a violent impulse. The only thing that I might have supposed existed now, was a softened state of the heart. I felt satisfied that such sudden debility would not take place after marks of inflammation or signs of effusion, without perfect atony, or softening, perfect loss of power in some one organ, and that organ might be the heart; but as he had a constant feeling of debility at the pit of the stomach, and great flatulence and thirst, I concluded that the stomach might be softened; and on opening him I found that the whole of the interior was of a dark-red colour, congested and pulpy, and that there was an ulcer as large as a shilling at one point, and which I had no doubt, from the appearances altogether, and from the ulcer itself being superficial, had not existed above ten days before death. We frequently see ulceration of the glans penis, from little more than irritability of the skin, and which has not existed more than a few days. This ulcer was merely on the surface of the stomach, and you might have scraped any part of the surface off.

Hence you see that cases arise where the ear is of no service, but other symptoms alone may at times be equally deceptive. If you take the pulse, in any disease, and only attend to that, you may find it quick, merely because the person has taken some stimuli just before, and you find it slow in some people, not from any particular state of the body, but merely because the stomach is out of order, in consequence of the person having taken something which has disagreed with him. On the other hand you will find a healthy pulsation in some people a bad sign. Some persons will have an intermittent pulse in health, and when they are ill it will become regular. There is no valid objection to auscultation because sometimes it will not afford you any information. There is no one symptom that will not lead you into error, if you do not attend to other symptoms at the same time. A philosophic man will observe every fact, will collect evidence by all his senses, and compare all he collects.

NON-REMUNERATION OF SURGEONS.

To the Editor of THE LANCET.

SIR,—There has been so much squabbling lately between the different public journals about the "*immense*" sums that medical men receive for attendance upon cholera patients, that I trust no further excuse will be required for the following:—

The two gentlemen who have attended at the Cholera Hospital in Winchester-street, in the Borough, for the last month, have not received one farthing!!! They are, Messrs. Gaselee and Tweedie,—two as indefatigable young men as ever the Borough produced.

The latter gentleman is now appointed to the Cholera Hospital in Abchurch-lane. He has not stated to me what he receives for his services there, but the former statement I had from his own lips. P. P. P.

ST. GEORGE'S HOSPITAL.

CLINICAL REMARKS

MADE BY

MR. BRODIE.

Monday, March 19 and 26, 1832.

TYING OF HEMORRHOIDS.

I AM going, Gentlemen, to-day to tie some piles, an operation which you have not the opportunity of seeing very often in the hospital. It is one, however, which you will be often called on to perform in private practice, as the modes of living amongst the higher classes predispose them much more to this complaint than the poor. It is a very safe operation. I never lost more than one case in my private practice, of probably more than three hundred cases in which I have performed the operation. The case which proved fatal was one in which I was *obliged* to perform the operation, as the patient was used to lose large quantities of blood upon going to stool; and in this case, on which I am now about to operate, Mr. Fernandez informs me that

the patient loses about a pint of blood every time he goes to the water-closet. The man is very much blanched in countenance from this excessive hæmorrhage, and being naturally of a dark complexion, he has acquired a sallow, miserable appearance, strongly indicative of debility and suffering. He has also some symptoms of affection of the chest; but it is impossible in his present state to judge how far these go. It is rather a dangerous remedy to tie internal piles; but external piles you may always tie with safety. I used at one time to cut external piles, and this I was led to do from having met with a copy of Mr. Cline's lectures, in which he says, "that cutting off piles is the most safe and certain way of ridding the patient of them; and that none but the most timid surgeons ever tie them; and knowing Mr. Cline to have been a most cautious and careful man in practice, I followed his advice, and cut off piles in several instances; but I was soon obliged to desist from this plan of treatment, owing to the excessive and alarming hæmorrhage which came on afterwards. Since then I have always *tied* piles.

HYSTERIC (!) AFFECTION OF THE KNEE.

In a case of inflamed bursa under the ligamentum patellæ, which had been reduced by blistering, but in which the patient still complained of much pain, Mr. Brodie remarked that the affection which remained about the knee partook more of an hysterical character than anything else. The patient appeared to shrink when the skin over the knee was pinched up between the fingers, or even before the skin was touched at all. She had also all the appearance of an hysterical person, and the menstrual secretion had never been in proper quantity. Mr. Brodie ordered her

Inf. valerianæ (*Ph. D.*) ʒiss;
Tinct. valerianæ ammon. ʒj; *ter in die.*

On the 26th this patient was evidently much improved from the valerian; the pain in the knee had almost entirely left her; and Mr. Brodie, in speaking of the use of valerian in these cases, remarked, that Sir Henry Halford, who was "a dexterous prescriber," had a most excellent method of ordering this medicine, which was, in making the infusion, to pour boiling decoction of bark on the valerian root, and letting it stand until it get cold.

STRICTURE OF THE URETHRA.

It is very singular (remarked Mr. Brodie) that, although only on Friday last I passed a good-sized bougie, and let it remain for some minutes in the urethra, that now (Monday) it is so firm again, that I cannot pass even a small bougie or a small catheter through it. This man's stricture is situated in the anterior part of the canal, and these strictures are very difficult of cure. They are in general permanent strictures, and are composed of a thick gristly induration of the lining membrane of the urethra; those which are situated farther back in the membranous portion of the urethra, are generally spasmodic and permanent too. The best cure for these gristly indurated strictures, is to introduce a catheter, and cut down upon them at once. To a question asked by some of the pupils, Mr. Brodie replied, that Mr. Stafford's method of curing strictures would not do for these; for in dividing the stricture, you might cut into some of the cells of the corpus spongeosum of the penis, and if the urine get infiltrated there, bad symptoms might arise. Caustic (Mr. Brodie said) was only useful in cases of spasmodic stricture, such as occurred in the membranous portions of the urethra.

HOSPITAL GANGRENE.—I believe hospital gangrene to be nothing more than a common gangrenous ulcer. It derives its name of hospital gangrene more from the circumstance of its appearing frequently in hospitals, than anything else. I have never found it differ in its character from other gangrenous sores.

ABSCESS IN THE AXILLA.—An abscess will never heal (remarked Mr. Brodie) unless the opening into it is of sufficient magnitude to allow the matter in it to be discharged *as fast as it is formed*.

ANEURISM.—When the sac of an aneurism sloughs, the aneurism is always cured, but only by the artery being plugged up above the ligature by a long coagulum. When the old operation for aneurism was performed some years ago, the patients never recovered, for Mr. Pott expressly remarks, that in cases of aneurism it was always thought best surgery to amputate the limb. And the only cases in which the old surgeons would perform the operation (remarked Mr. Brodie), were those of popliteal aneurism, for which they made an incision down through the skin and muscle, over the artery, without any minute dissection, then ran a needle almost at random through it, and tied it.

DIVIDING THE SPHINCTER ANI.—After performing this operation on a patient, Mr. Brodie remarked, that he should not apply any dressing—merely a poultice. He had a case in private practice of this kind where there were a great many sinuses; but he only applied a poultice, and the case went on very well, and quite recovered. In dividing the sphincter ani muscle for the cure of fistula in ano (said Mr. Brodie) you should completely cut the muscle through, and then the contractions of it only serve to draw the cut edges further apart. If, how-

over, you only partially divide the muscle, the contractions will draw the divided surfaces into closer contact.

ANEURISMAL BLOOD.—In a case of aneurism of the aorta, which is diminishing in size anteriorly under the use of the digitalis and small bleedings, ʒii or ʒvi at a time, Mr. Brodie asked whether the blood last drawn exhibited any inflammatory appearance; and on Mr. Fernandez replying that it had, Mr. Brodie said that it was a remarkable fact, but that aneurismal blood, when drawn from the arm, always bore the marks and showed the traces of inflammation.

Report on the Chemical Pathology of the Malignant Cholera, published by Authority of the Central Board of Health. By W. B. O'SHAUGHNESSY, M.D. London. Highley. 1832. 8vo. pp. 72.

In the following article we shall lay before our readers a copious analysis of the above report, the publication of which, the author tells us in his preface, has been delayed several weeks, in order that he might be afforded the opportunity of previously investigating the nature of the *London epidemic*.

The work is divided into three sections, to which is subjoined an Appendix. Of each of these divisions we shall give such an outline as will enable the most superficial reader to form a just opinion of the nature of the author's labours.

In the first section Dr. O'Shaughnessy professes to treat of the chemical constitution of the human blood in the normal or standard condition, and he sets out with the statement of his belief that in any inquiry into the chemical pathology of a disease, in which a presumed alteration of the blood from the normal condition becomes a subject of investigation, it is essentially necessary to start with well-defined ideas regarding its standard composition. At present, too, it appears that a rapid review of the state of our knowledge on this subject is far from being superfluous, six months having scarcely elapsed since the publication of a most important and original essay on the subject, by M. Lecanu of Paris, an experimentalist who has devoted himself assiduously to the study of the chemistry of the blood, and who has modified many of the opinions generally entertained regarding that fluid.

Dr. O'Shaughnessy, in his review of the elements of blood in the healthy subject, divides them into three principal groups:—

"First, those invariably present, in a proportion little varying from a certain standard, and universally recognised by all chemists and physiologists. Secondly, those usually present, but occasionally absent, and perpetually liable to alteration in their quantity. Thirdly, those substances, the presence or absence of which in standard blood is asserted by some authorities and denied, or not recognised, by others.

"Under the *first* head may be included *water, albumen, fibrine, colouring matter, extractive matter*, and various *saline substances*; viz. the carbonate of soda, muriates, sulphates and phosphates of soda and potassa, carbonates of lime or magnesia: phosphates of these earths, and minute quantities of *iron* in an unknown state of combination.

"Under the *second* head may be specified *a fatty compound*, consisting of an oily and crystallisable matter; also *urea*, or the peculiar animal principle of urine.

"The *third* embraces a considerable number of substances: namely, the FREE *acetic acid, carbonic acid, cholesterine*, FREE *carbon*, and traces of *manganese, silica*, and *copper*."

He then proceeds to offer some observations on the functions of each of the principal ingredients comprised in these classes. The *colouring matter* first engaged his attention. Regarding this, he shows, that (the precise nature of this substance being yet quite open for investigation, and its proximate composition being not yet satisfactorily unravelled) we are not entitled to reason *positively* and absolutely on secondary phenomena manifested during the actions which take place between it and the atmospheric air. He further adduces Dr. Stevens's experiments as proofs that the saline matters of the blood are materially engaged in producing those changes, in which it was till lately believed, that the air and the colouring matter were exclusively concerned. In illustration of this fact, we may remind our readers that *black blood* cannot become *scarlet* by exposure to the air, unless it contain *saline* matters.

Passing over the second group, we proceed to the third, or those the presence or absence of which still rests on questionable testimony. In this division the author includes cholesterine, osmezome, cruorine

free carbon, acetic acid, carbonic acid, silica, manganese, and *copper*. In our analysis we shall restrict ourselves to the consideration of the substances in *italic* characters, and begin with the acetic acid, an important topic for consideration, "an analyst of some reputation, M. Herman of Moscow, having recently, at the same time, announced the discovery of this substance in healthy blood, and endeavoured to establish on this supposed fact some very peculiar opinions on the chemical pathology of the malignant cholera."

Dr. O'Shaughnessy first gives a faithful translation of the experimental part of Herrmann's paper, and then proceeds to show that these experiments are altogether fallacious, from a critical consideration of the experiments themselves. He then gives the following direct evidence in disproof of Hermann's opinions:—

"But to satisfy my mind directly on the matter, I submitted to examination five specimens of blood, drawn from perfectly healthy individuals. I allowed the blood to separate into serum and coagulum in *close* vessels; I diluted the serum largely with distilled water, and brought the mixture to the boiling point in a distilling apparatus. The product in the receiver was perfectly neutral, did not affect test papers of any description, was rendered alkaline by the addition of a single drop of a dilute solution of soda, and when subsequently evaporated to dryness, left a minute saline residuum, containing not one particle of any combination of acetic acid.

"The clot was similarly treated, and with the same results.

"Again, a paper having been published about three months since, by M. Orfila, in the *Annales d'Hygeine publique*, on "*poisoning by* ACETIC *acid*," I instituted several experiments to ascertain the minutest quantity of that acid which I could detect in the blood, and I found that a quantity so small as to do little more than neutralize the natural alkali of the serum, could be satisfactorily recovered from any proportion of blood by the processes I have just described, and which I would follow exactly in any medico-legal inquiry."

From the evidence of these facts, he considers himself fully justified in stating, "that M. Hermann has failed to establish the existence of *free* acetic acid in healthy blood, and that he is consequently not entitled to resume his analysis as a standard for the estimation of the alterations in that fluid, effected by, or producing decreased actions in, the general system.

The author next proceeds to notice the disputes regarding the existence of carbonic acid in venous blood, and he expresses his belief in the affirmative. The origin of this carbonic acid next engages his attention, and he successively reviews the several theories devised for its application. He rejects the opinions of those who considered it to arise from the combination of free carbon with oxygen *in the lungs*, and its consequent reabsorption into the circulation. He also rejects, on the ground of insufficient proof, the hypothesis of Dr. Clanny, who believes it to proceed from "assimilation" alone. He then states his own opinion on the question in these terms:—

"The third opinion regarding the carbonic acid of the venous blood is one which I myself would venture to throw out, and which constitutes a kind of patch-work, composed of the ideas of Edwards, Stevens, Crawford, Hussenbratz, and La Grange. I believe it to arise principally from the process by which the *detritus* of the system is removed. The carbon and hydrogen of the renovated or changing parts, thus combine with oxygen, supplied them through the intermediate agency of the colouring matter of the blood, which, perhaps with the co-operation of the saline matter, acts as the vehicle in which the oxygen is conveyed from the air-cells of the lungs. Carbonic acid and water are thus formed,—the arterial blood merges into the venous kind, and is returned to the pulmonary circulation. Here, by an action of aerial "*endosmose*" and "*exosmose*," the carbonic acid is exchanged for oxygen, and the same circle of actions is renewed.

"Before this opinion can be admitted, I frankly allow that the proximate nature and ultimate constitution of the sanguineous colouring matter must be much more minutely ascertained. It is not necessary, as some have imagined, that arterial blood should be found to contain more oxygen than venous blood; for the oxygen is not absent in the latter, it has only changed from the colouring matter, to the condition of carbonic acid or water. It would, however, be essential to show that the arterial colouring matter contained more oxygen than that of the venous blood. That such is actually the case, is rendered highly probable by the experiments of M. F. Michaelis, recorded in his *Inaugural Dissertation*, presented last year to the *University of Berlin*. According to his ultimate analysis, the colouring matter

of venous blood contains much less oxygen than that of arterial blood."

From the general state of our knowledge regarding the source of carbonic acid, Dr. O'Shaughnessy concludes, that we are not entitled to speak, either of its origin or functions, in positive terms; consequently, that its absence in a state of disease cannot be assumed as a *cause* of the deranged condition, and at most can only be regarded as an *effect*, of very questionable importance.

The next *reputed* ingredient which the author considers, is FREE *carbon*,—that to which, to use his expressions, so many *theorists* have ascribed, and still attribute, the black colour of venous blood. The author, however, denies that any positive experimental proof has yet been afforded of its presence in that fluid.

"Fondly as this idea has been cherished by anatomical physiologists, no satisfactory experimental proof has hitherto been afforded of the existence of carbon in the blood, in any other form than that of a binary, ternary, or even quaternary combination. No mention of any such ingredient occurs in the tables of analytic results given by Berzelius, Marcet, Denis, or Lecanu; but within the past week I find, to my surprise, that Dr. Clanny has, in a table of the results of a comparative analysis of a specimen of blood drawn from a healthy person, and of another taken from an individual labouring under malignant cholera, not only enumerated FREE *carbon* as an ingredient in both cases, but even declares the *quantity* to have been 32° in the healthy, and 66° in the morbid example. I am exceedingly anxious to learn by what direct mode of analysis this extraordinary result was obtained, but until the experiments are published in full and satisfactory detail, I am not warranted in recognising the existence of the principle in question, and I am compelled to receive with caution all reasoning founded on its alleged presence in healthy, or its preponderance in morbid, blood."

Of the remaining substances described by some writers, as constituents of the blood, viz. copper, manganese, and silica, the author takes but a very cursory notice. The claim of copper to be introduced into the list being principally supported by experiments performed several months since by Dr. O'Shaughnessy himself, and described in a paper which he communicated to this journal, the author, in the report before us, merely mentions these researches, at the same time candidly admitting that they require to be more extensively repeated. He states:—

"I do not therefore believe that copper should be for the present inserted in the tables of our analysis of the blood. I trust the readiness with which I make this concession, will exonerate me from the imputation of entertaining any illiberal feeling towards the experimentalists, whose analyses I have had, or shall have, occasion to criticise in this report.

"The remarks I have made are equally applicable to the only remaining substances, viz., silica and manganese, both of which, as well as alumina, can be separated in traces, from very large quantities of blood."

Having thus sufficiently noticed all those elements of the blood concerning which any controversy still exists, the author then touches on the important topic of the relative and quantitative proportions of the several ingredients whose existence is *universally admitted*. To this he requests the special attention of the pathologist, inasmuch as his subsequent experiments denote, that in cholera an absence or deficiency of some of the ingredients of the blood, and a remarkable deviation from the normal proportion in others, constitute the most remarkable features in the chemical phenomena of the disease.

Dr. O'Shaughnessy selects M. Lecanu's analysis of normal blood as the standard by which he estimates the constitution of that fluid in cholera. He accords to M. Lecanu the preference, from the opinion, that his processes are perfectly free from fallacy, and incomparably the best ever devised for the quantitative and qualitative analysis of the blood. They have been repeated on a multitude of individuals of all ages, of both sexes, and temperaments, and have been rewarded by the gold medal of the *Académie de Médecine*, conferred at the public sitting of July 1831. A standard such as this, evidently afforded the best guide for the estimation of the state of blood in cholera, especially when *the reporter pursued precisely the same processes of analysis which M. Lecanu had followed*.

We now arrive at the second section, in which Dr. O'Shaughnessy presents a review of all the experimental researches (including his own), conducted on the

blood, dejections, &c., in the malignant cholera; namely those by Dr. Turnbull Christie, Hermann of Moscow, M. Foy at Warsaw, and Dr. Clanny at Sunderland. Dr. Christie's experiments having been confined to the dejected matters, are passed over very briefly, and the author next subjects those of Hermann to an elaborate critical examination, which, we believe, will fully convince the reader of the slight degree of value to be reposed on Hermann's results and opinions. Dr. O'Shaughnessy then proceeds to describe the results of his own inquiries, an outline of which was published in this Journal on the 31st of December 1831. These results, he informs us, were based on the examination of three excellent specimens of blood drawn in the malignant cholera, one in a case of violent bilious diarrhœa, and thirdly, of the dejections of one of the patients who died of malignant cholera, and whose blood was also examined. In describing the results of the analyses of the blood, the author gives a history of each patient from whom a specimen was drawn, and then in a set of tables contrasts the quantitative amount of the respective ingredients, with the normal standard afforded by the analysis of Lecanu.

In the first case, the serum of the blood was alone examined; its sp. gr. was 1.041. That of Lecanu's normal table is 1.028. On analysis, the quantity of water in the cholera serum, was found to be 854 parts in 1000; healthy standard, 90.6 of albumen; in the cholera serum, 133 parts, healthy standard 78; of urea, cholera serum 0.40; healthy standard .0; saline matters, cholera serum, 5.60, healthy standard 11.; carbonate of soda, chol. serum, .0,* healthy standard (along with phosphate and sulphate) 2.10.

"The tabular view thus afforded of the results in this analysis, is interesting in several points of consideration. It shows, in the first place, the absence of a large proportion of water; secondly, the corresponding preponderance of albumen; thirdly, the presence of urea; fourth, the absence of the alkaline carbonate; fifth, a great *deficiency* of saline materials. I should also add, that the experiments which gave origin to this table, were witnessed by Dr. White of Newcastle; Mr. Hawthorn, the nephew of Dr. Macwhirter, was present when the urea was obtained."

The second case and analysis are of an example of *violent common bilious diarrhœa*. The observations of the author are here of considerable interest:—

"The second case which afforded me materials for continuing the inquiry, occurred in the practice of Mr. Hawthorn; it was one of very severe bilious and feculent diarrhœa with vomiting. I gladly embraced the opportunity of examining this case, as I was exceedingly anxious to ascertain whether the loss of water in the blood was an invariable sequel of profuse intestinal evacuations. The result shows the singular fact, that the fluid parts of the blood were, on the other hand, *increased* in this instance. The consideration of this phenomenon, and of some other peculiarities of the dejections in the malignant cholera, to which I shall advert in the sequel, will probably furnish at no distant period a diagnostic test of chemical accuracy, by which the indigenous cholera of Great Britain to which we have ever been accustomed, can be distinguished from the disease of the same name with which the present winter has, for the first time, made us too familiar."

A table is then given, in which Lecanu's standard, the first analysis in malignant cholera, and that in the bilious diarrhœa, are contrasted, and from which we learn that in the last, the *water exceeded even the natural standard*; the albumen was below the average proportion; the salts were normal in quantity, and the serum preserved its alkaline character.

The third case and analysis is the most important of the whole, inasmuch as the individual who afforded the blood, laboured under cholera in its most malignant form; the dejections he passed were also obtained for analysis, and that before any medicine was given.

The crassamentum of the blood was examined according to Lecanu's process, and found normal in the proportion of its ingredients. The serum and coagulum were in the proportion of 43 serum and 57 crassamentum (normal standard, 57 serum, 43 crass.), and the serum of the sp. gr. 1.45 (normal stand. 1.29), all which circumstances indicated an extraordinary loss in the aqueous portion of the blood examined.

* In this table we observe that *potassa* is erroneously mentioned instead of *soda*, but it is evidently a misprint.

The serum, moreover, was destitute of the normal alkaline action, which indicated a deficiency in the saline matters; accordingly on accurate analysis, this serum was found to contain but 2.92 of salts, per 1000 parts, the normal quantity being between 11 and 12.

The analysis of the dejections in this case may now be considered with advantage. The fluid portions were strongly alkaline, and contained water, carbonate of soda, and traces of albumen, the solid flabby portion again consisted chiefly of fibrine, so that the ingredients defective in the blood were detected in the dejections, or in other words, *the addition of the dejection to the blood, in due proportion, would have restored the latter to its normal constitution.*

Dr. O'Shaughnessy then proceeds to show by microscopic experiments that the blood in cholera retains its globular or anatomical structure. He further proves that it is capable of discharging its functions in the act of respiration, as far as the power of decomposing atmospheric air is concerned. The proof of this important fact is afforded by a repetition of Dr. Christison's beautiful experiments, long since described in this Journal.

The author next briefly notices the papers on this subject published by Dr. Clanny, whose results he does not deem satisfactory, and he then passes on to the third section of his report, in which he inquires into the extent of the pathological and therapeutic conclusions deducible from the preceding statements. To this section of the essay we would call the particular attention of our readers, inasmuch as it is a strictly logical series of deductions, disfigured neither by empty hypotheses, nor by untenable and wild speculations. This is what we want in medical reasoning. Let nothing be argued upon until it is proved; let no fact be received until its truth is established; then, and not till then, will medicine rank as a science, and the disputes and cavillings of its professors become susceptible of adjustment according to exact and incontrovertible data.

The author thus commences the section in question:—

"In order to reason correctly on the indications afforded by a series of asserted facts, it is absolutely necessary, first, that the reality of these facts should be ascertained; and, secondly, that their universality should be determined. Thus alone can we be entitled to regard such facts in the light of *essential causes* or *essential effects*, and to consider them as distinct from any *accidental* phenomena. Again, even though the veracity and universality of the facts in question shall have been satisfactorily established, it will still remain a subject for investigation whether it be to the class of 'causes' or to that of 'effects' that they are to be referred, and if to the latter, whether they are essentially or intimately connected with the progress of the malady, or whether they are merely the insignificant though constant, companions of other more important, though less obvious, circumstances. Unless we follow this strict system of ratiocination in all inquiries like the present, though we may accidentally stumble upon true conclusions, we can never reach them by a certain path, or by one which subsequent inquirers can ever unhesitatingly and effectually pursue."

The necessary postulate *of the veracity of the facts asserted*, the author founds on the strong ground of the experiments having been performed before competent witnesses.

The second postulate, *as to their universality*, he assumes hypothetically in the report itself. In the appendix we find, however, that he considers it perfectly established, accounts having reached England since his report was laid before the Board of Health, of the results of the experiments performed by the celebrated Rose at Berlin, which correspond almost to a fraction with Dr. O'Shaughnessy's statements. From the appendix we also learn that in the interval which has elapsed between the presentation of the report and its publication, the author has examined several specimens of blood and dejections drawn from persons labouring under the cholera in London, *and found the same constitution of the blood and dejections to exist.* The "universality" of the facts regarding the chemical pathology of cholera is thus incontrovertibly decided.

Admitting, then, the veracity and universality of these facts, he next inquires, are they to be regarded as *causes*, or as *effects*, of the disease?

"That they should not be regarded as primary *causes* is rendered evident by the fact that the blood *per se* not being endowed with an inherent power of locomotion, can-

not be altered in *quantity* within the vessel by any internal force; the moving power must proceed from without. Neither, I believe, can its *quality*, while life exists, be impaired by any really intrinsic causes. External means may, however, during life operate powerfully upon it, either by the introduction of substances alien to its constitution, or by the subtraction from or addition to the amount of the ingredients it naturally contains. It is, moreover, rendered evident by pathological facts, to which I need not particularly advert, that the vessels in which the blood is contained, or the nerves distributed on these vessels, influence both the proportionate and the qualitative state of the contained fluid, in a manner which, in the present state of knowledge, we cannot satisfactorily explain, even though aided by the light which the brilliant discoveries of Dutrochet have shed upon the nature of increased vascular action.

"Regarding, then, as we must, this alteration of the blood as the *result* of an external impression, we still remain in darkness as to the mode in which that impression is communicated,—of its nature we have more knowledge to a certain extent; every circumstance in the semeiology and anatomical pathology of the malady, the kind of its symptoms, the sequence in which they occur, and 'period of incubation' between the application of the primary cause and the appearance of morbid symptoms, all coincide in denoting that an organic poison is the first mover of the consecutive actions."

The absence of water and saline matters in the blood being thus regarded as *effects*, are these effects *essentially* connected with the progress or the events of the malady?*

"The most obvious manner in which the loss of water could occasion death, is by the physical obstruction it would necessarily occasion in the passage of the blood through the capillary vessels, especially in the pulmonary circulation. The motion of the blood would, therefore, be confined to the great vessels, which would become distended to excess—gradual asphyxia should supervene, and death be occasioned with all the phenomena of impeded respiration and circulation. How accurately this description coincides with the events in cholera, it is unnecessary for me to point out. In short, this mode of death in this disease might at once be admitted, had we previously accurate ideas regarding the precise density of fluid which the capillary system will receive and permit the passage of, and had we found that the density of cholera blood exceeded this standard. I regret that my inquiries among some distinguished anatomical friends have failed to procure me any conclusive evidence on this subject. I shall therefore content myself by expressing my conviction that this deficiency of water in the blood, is, at any rate, in many cases *a powerful adjuvant* in the production of the fatal event." * * * "Giving thus to probable mechanical hyperæmia as much importance as I think it is entitled to receive in the consideration of this disease, I also cannot, or will not, conceal my conviction that the remote cause of cholera *may*, and FREQUENTLY DOES *produce death, independently of this auxiliary, by the unknown agency it exerts on the nervous system.*"

And regarding the probable effect of the absence of the saline matters, he observes—

"I should add, that from all I have been able to learn on the subject, the diminution in quantity of saline matter in the blood is not immediately incompatible with life, and that the injury resulting is rather of a chronic character—that is to say, that it takes hours or days for its production; I consequently would not attribute to the absence of the salts any important share in the inducement of sudden death in cholera, but I am inclined to believe such diminution or absence to be closely connected with the fever stage of the malady, an opinion at the same time suggested and substantiated by Dr. Stevens's experiments on the state of the blood in the yellow fever of Santa Cruz."

We now arrive at the last department of the essay—viz. the therapeutic conclusions. The indications of cure seemingly dependent on the preceding facts, are two in number—viz. 1st, to restore the blood to its natural specific gravity; 2nd, to restore its deficient saline matters.

"The first of these can only be effected by absorption, by imbibition, or by the injection of aqueous fluid into the veins. The same remarks, with sufficiently obvious modifications, apply to the second."

In the milder cases, or preliminary diarrhœa, he considers astringents of great value, inasmuch as they may prevent the inspissation of the blood by the alvine discharges. From his experience in the treatment of the Edinburgh dysentery of 1829, he prefers a combination of opium and acetate of lead to any other formula of this description. In severer cases copious ene-

* Before answering the question, the author reminds his readers that water and saline matters may find many outlets from the body, independently of vomiting and purging; for example, by the kidneys and skin; and he notices the curious fact of excessive diuresis having been prevalent among the medical gentlemen attendant on cholera cases in Newcastle.

mats of warm water, holding the natural salts of the blood in solution, are strongly recommended, a measure which has been actually practised with much benefit in the north of England, but the suggestion of which, in the treatment of severe English cholera, is as old as the days of Sydenham.

When absorption is entirely suspended, as in those desperate cases which are unhappily now of daily occurrence in this metropolis, the author recommends the injection into the veins of tepid water, holding a solution of the normal salts of the blood; his experiments having, we presume, led him to abandon his former ideas respecting the superior efficacy of highly oxygenated salts for this purpose.

We have now completed our analysis of this report. The appendix consists of six sections, five of which are devoted to the details of the chemical manipulation employed in the experiments, and to lucid instructions, according to which the analysis may be repeated by persons little versed in chemical experiment. The last article of the appendix contains the results of Rose's experiments in Berlin, to which we have already adverted; and also the summary of the author's own experiments instituted since the epidemic first appeared in London.

"The identity of the chemical pathology of the cholera of Berlin and Newcastle being thus fully proved, I was naturally anxious for an opportunity of examining into the state of the blood and the alvine dejections in the disease now prevailing in London. I felt that my report could not be complete, did it not comprise this important feature; and I also felt that the results of my experiments would throw great light on, and perhaps altogether decide, the professional controversies, which the irruption of the disease in London has unhappily occasioned.

"I have now completed the analyses of four specimens of blood, drawn from persons labouring under this disease in its severest forms, and of eighteen examples of the peculiar dejections, obtained from patients in different quarters of London.

"The results correspond so perfectly in every particular with those of the Newcastle and Berlin analyses, that to describe them would be but the repetition of the same terms.

"I have also availed myself of the interval which has elapsed between my return from the north of England and the publication of this report, to examine still further whether in ordinary diarrhœa the blood experiences this particular change, and whether the dejections ever present the same properties in any other disease or condition of the system.

"In all the cases of ordinary diarrhœa I had an opportunity of examining, amounting to seven in number, the blood preserved its normal proportions.

"Of more than 100 cases of ordinary diarrhœa, the dejections presented no single property of the cholera evacuation.

"Artificially I found in one experiment, that the administration of a powerful saline cathartic to a person while fasting, can produce the transudation of part of the constituents of the blood, rendering the dejections similar to the cholera character in kind, but widely different in the *degree* and quantity of the transudation.

"I am therefore entitled to conclude, that the exudation of the colourless part of the blood constitutes one of the chief diagnostic characters of the malignant cholera.

"That the inspissation of the blood is essential to its pathology.

"*That these conditions being alike in the diseases of* Berlin, Newcastle, and London, *the diseases in these three situations are entirely the same*; and lastly,

"That in the fluidity, alkalescence, and albuminous nature of the dejections, we have the means of forming a certain and chemical diagnosis between this disease and others, with which, on a general consideration of ordinary symptoms, it may be confounded. The chemical re-agents for the examination of these conditions are—1st. Turmeric paper, which is turned brown by the dejections. 2nd. Boiling in a glass tube or flask, by which the albumen is coagulated. It is necessary to state that these conditions are, of course, more remarkable in the early period of the disease. When the diarrhœa has persisted for some hours, the alkaline character is frequently lost, from the simple fact of all the alkali of the blood having been passed away. The quantity of albumen also declines for a parallel reason, and requires the test of ferro-cyanate of potash for its detection."

We have abstained from offering a word of comment on this essay, until we had given our readers a full view of its most prominent features, in order that they may be enabled to decide, whether, in recommending it to the general perusal of the profession, we manifest the least degree of partiality towards its author. Although professedly on a chemical subject, the details of the report are yet so separated from the nomenclature of chemical mani-

pulation, that they become intelligible even to persons who possess little or no chemical knowledge.

On the whole we regard it as an essay instructive in several points, highly curious as an example of the successful revival of a *part* of the *humoral doctrines*, and, lastly, important as deciding, beyond dispute, the *identity* of the diseases of Warsaw, Moscow, Berlin, Sunderland, Newcastle, and London; and the difference, the essential *pathological* difference which exists between the bilious cholera common in this country, and that fearful disease which has recently made this island the scene of its terrible visitation.

THE LANCET.

London, Saturday, March 31, 1832.

No proposition for an act of parliament, however monstrous or absurd, can be made in the House of Commons, without finding supporters in that assembly, and also amongst individuals "out of doors," who have the modesty to class themselves with the "well-informed portion" of the public.

Mr. WARBURTON'S Anatomy Bill is to go to the Lords! A bill which provides that not less than *three* inspectors of anatomy shall be appointed for England and Scotland, and that of such inspectors one at least shall reside in London and one in Edinburgh! As well might the legislature appoint three "grave diggers." Has it not already been a thousand times proved that murder may be so committed as to evade detection in post-mortem examinations? Is it not known to every medical practitioner, in fact to every person in the kingdom except *professional lawyers*, that were the first anatomists in Europe appointed inspectors, they would be incapable of proving from an inspection of the bodies merely, that the individuals had come to their death by force or poison. It ought to be recollected that not less than *sixteen* murdered bodies were purchased and dissected in *one* celebrated school of anatomy in Edinburgh, and in no instance were the evidences of the violence discovered. But, it will perhaps be replied, "The 'inspectors' will always take care that bodies shall not be received for dissection, unless accompanied by the 'certificates' prescribed by the bill." A goodly occupation truly, and a really *efficient* one, considering that this officer will in no respect be enabled to protect himself from imposition, more effectually than the receiver or purchaser of the body. Besides, would a murderer hesitate to commit forgery? A contrary supposition would touch the extreme point of absurdity.

At present we want but two things—first, the repeal of that portion of our criminal law which consigns the bodies of murderers to dissection, which the bill provides; and secondly, an enactment to prohibit, under some *dreaded* and *certain penalty*, the buying and selling of dead human bodies. In these two simple propositions we strike at the root of every prejudice felt by the public on this subject, and at the same time provide for the speedy and everlasting annihilation of the slaughterers and resurrectionists. If there could be no *sale*, there would be no temptation; but now (ingenious device!) in order to get rid of the inducement to commit murder for the sake of the sum at which the body may be disposed of, we are called upon to *legalize* the *sale*. Oh no! we beg pardon: as Mr. PLACE and the Rev. —— FOX, together with some half dozen barristers, belonging to the Council of the NATIONAL POLITICAL UNION, boldly tell us that Mr. WARBURTON'S bill does not *legalise* the sale of dead bodies. Upon what clause or statement of the measure do they found this assertion, for argument it is not? Why, upon the *omission* of every word relating to the sale of bodies; that identical *omission* which, we contend, must have

the effect of legalising this most disgusting and dangerous traffic. The principles of legislation in this country are already sufficiently defective and pernicious, without requiring that there should be a *special* provision in some act of parliament for rendering every transaction *legal*. As well might it be inferred that it is *not* lawful to sell an ox, a horse, or a cabbage, without the protecting authority of an act of parliament.

Again, it is contended by the *lawyers* of the Union that, "according to the existing laws, it is a misdemeanour to do anything with dead bodies, but wash and bury them; even it is criminal to give our own bodies for dissection, according to the report of the anatomy committee of the House of Commons; for an indictment can be framed, and a conviction follow. The non-execution of those laws does not prove their non-existence. Magistrates may interpose, though they do not."

Never was there a more complete begging of a question. This is almost as ludicrous as the assertion, that the bill does not legalise the sale of dead bodies, "because it does not say so." We marvel that a barrister should dig for his law in the report of an *Anatomy Committee*. The non-execution of the law certainly does not prove its non-existence. But what are we to infer from its *non-production*? Why, correctly enough, that no law to punish a surgeon for dissecting a body of which he may become legally possessed, *is* in *existence*. A pretty doctrine!—a father may be punished for allowing a surgeon to examine the *dead* body of his child, whilst all the world knows that he is not responsible for allowing him to operate upon the *living* body of his offspring!

If such laws really exist, let them be repealed; but we ought not to legislate under a conviction that they do exist, unless our eyes be furnished with indisputable evidence of the fact.

Great and manifold will be the evils which this bill will produce, if it be passed into a law. Instead of tranquillising the minds of the poor, it will render them furious from a belief that the overseers and governors of workhouses are empowered to dispose of their bodies to the anatomists for profit, and hence the most serious and dangerous riots will result. The "monopoly schools" will, under such an act, receive additional encouragement, and command increased facilities for depressing less wealthy private establishments: further, by rendering the sale of dead human bodies an affair of legal traffic, every obstacle to crime will be removed from the minds and feelings of those reckless villains who, for the smallest amount of gain, are ever ready to plunge a dagger into the hearts of their fellow-creatures.

The steady progress of the wide-spreading pestilence is a circumstance calculated to deter the mind from indulging in any expressions of exultation over defeated and refuted contemporaries. There cannot be a reader of the public journals, who has forgotten the virulence with which we were assailed for having been the first to announce to the public that the cholera had extended its poison to this metropolis. In open defiance of fact, in wilful opposition to truth, in spite of the victims which daily sank into the grave, was it audaciously and infamously alleged that The Lancet was labouring to deceive the public, and endeavouring to create unnecessary alarm. With what motives, or with what views, was not explained. This was dangerous ground, and it was carefully avoided. With what motives our contemporaries laboured so sedulously to convince the public that they were free from danger, while yet the enemy was at the very threshold of their doors, they must themselves furnish the explanation; we shall certainly attempt none for them, but trust their intentions

were as free from improper bias, as were those by which we ourselves were actuated. In forming abstract opinions, nothing is more easy than to fall into error. But what are we to say of men whose prejudice is so strong, that not only every avenue leading to the understanding is closed, but all the perceptions are obscured and perverted? What are we to say of individuals who, denying the existence of facts which were placed before their eyes, wilfully give currency to false statements, and attempt to build up theories upon lies of their own invention? Although furnishing testimony which falls to the lot of few publications of the sagacity, honesty, and intrepidity of this Journal, we nevertheless deplore having it in our power, at the conclusion of this volume, to lay before our readers such a summary as the following, showing at once the fearful character of the disease by which we are visited, and the unpardonable attempts at deception which have been practised upon this too confiding community.

From the 10th of February, the day on which the cholera was announced to exist in this metropolis, up to the 27th of March, there have been 1374 cases, 836 deaths, and 426 recoveries, 312 patients now being under treatment.

In the week commencing the 10th of February, and ending the 19th of the same month, there were 34 cases and 18 deaths.

In the week ending 26th of February, 36 cases and 20 deaths.

In the week ending 4th of March, 143 cases and 83 deaths.

In the week ending 11th of March, 277 cases and 139 deaths.

In the week ending 18th of March, 381 cases and 395 deaths.

In the week ending 25th of March, 389 cases and 225 deaths.

In the four days ending 27th of March, 313 cases and 156 deaths.

The localities in which the cases have taken place are as follows, with the totals of cases and deaths, as issued from the Council Office, according to the undermentioned date.

CENTRAL BOARD OF HEALTH,

Council Office, Whitehall, March 28.

Places.	Total Cases.	Total Deaths.
Mile End Old Town	2	1
Afloat on the River	83	33
Limehouse	25	19
Shadwell	20	17
Ratcliffe	10	7
Poplar	17	6
Bermondsey	116	22
Southwark	616	295
Newington Butts	95	46
Camberwell	0	0
Lambeth	116	85
Christchurch	58	28
Westminster	39	21
Chelsea	27	21
St. George's, Hanover-square	11	8
Marylebone	69	20
Paddington	0	0
St. Andrew, Holborn	3	2
St. Giles's	58	34
Whitechapel	62	42
St. George's-in-the-East	24	15
St. Pancras	10	8
Bethnal Green	7	5
Old Brentford	18	10
Wandsworth	8	3
Woolwich	0	0
St. James	1	1
St. Botolph	9	5
Wapping	4	3
Total	1530	802
Before reported	44	34
	1574	836

Places.	Total Cases.	Total Deaths.
Ely, March 27	22	9
Shouth Shields, March 26	18	8
Canon Mills, March 25	10	8
Edinburgh, March 25	45	25
Haddington, March 25	136	62
Water of Leith, March 24	49	26
Longloan, &c. March 23, 24	71	33
Porto Bello, Mar. 21, 22, 23	0	0
Preston Pans, March	0	0
Calder Ironworks, March 17 to 25	17	5
Greenock, March 25	46	23
Glasgow, &c., March 25	512	252
Paisley, March 25	282	159
Dumbarton, March 25	5	2

Coldstream, March 25	116 ..	41
Perth, March 25	35 ..	12
Total	1315	639
Total from places where the Disease has ceased, or from which no returns have been received to-day....	5680	1619
Grand Total	6995	2258

(Signed) W. Maclean, Sec.

LONDON HOSPITAL.

FUNGUS HÆMATODES OF THE HIP.

E. Repper, aged 34, married, mother of seven children, was admitted into this hospital on the 28th instant, labouring under fungus hæmatodes, situate on the right hip. She states, about seven months since she received a blow on the hip, to which cause she attributes the formation of this tumour. Shortly after receiving the blow a small hard lump formed, which very gradually increased until within the last three months; since which time it has rapidly increased in size. The tumour is of the size of a large orange, lobulated, with a patch of ulceration of the size of half a crown. Complains of severe lancinating pains in the tumour: the glands of the groin are free from enlargement.

On being informed of the necessity of submitting to an operation, she readily consented; and was, on the 29th February, brought into the operating theatre, when the tumour was removed by Mr. Luke, who commenced by making an incision round the tumour; after which it was easily removed from the gluteus maximus muscles to which it was attached. Twelve ligatures were applied; and the edges of the wound were then approximated as near as possible, by strips of adhesive plaster. The patient bore the operation with great fortitude.

Since the operation nothing of any importance has transpired in this case to require a daily report. She is going on very well, the wound looks remarkably healthy, but little suppuration has taken place, and she is allowed full diet, with beef tea, wine, and porter.

UNSUCCESSFUL OPERATION FOR HYDROCELE.

5th March.—This day Mr. Hamilton, one of the assistant-surgeons of the hospital, operated for hydrocele on a man aged 33. The trocar and canula were introduced about two inches from the most depending part of the tumour, and about 14 ounces of a straw-coloured fluid were evacuated. The tunica vaginalis was injected with a solution of the sulphate of zinc and water; which was withdrawn at the expiration of two minutes, the patient complaining of severe pain.

March 10th.—The operation in this case has not succeeded, the swelling having returned, and being now nearly as large as before the operation was performed.

CANCER LABII.

March 19.—A cancer was removed from the lower lip of Edward Bray, ætat. 61; it had existed for upwards of nine months, and began to ulcerate at its outer and superior surface. Mr. Hamilton, who operated for Mr. Andrews, removed the diseased part by making an incision obliquely downwards with a scalpel on one side of the diseased spot, the lip being extended by the lip forceps, and repeating the incision on the opposite side. A triangular piece of flesh containing the disease was then removed; of course the inferior coronary and labial arteries were cut through in the operation; three ligatures were applied, and the divided edges of the lip were brought in contact and retained by strips of adhesive plaster, without any suture being used.

25.—The lip was dressed to-day, and looked remarkably well, adhesion having taken place throughout the whole extent. The last of the ligatures was removed this day.

On the 19th also, Wm. Clarke, ætat. 23, was admitted with an injury of the right hand. States, that while employed on board a vessel, discharging some barilla, the jigger gave way, the handle of which struck his hand, causing a fracture and extensive laceration of the soft parts of the thumb,—so severe, indeed, as to require amputation of the thumb, which was immediately performed by the dresser.

WANT OF FRACTURE BEDS.

William Roberts, labourer, ætat. 49, was admitted March 22nd, with a compound fracture of the left tibia. The limb was placed in splints, and ordered to be kept wet with spirit lotion. This case would not have been noticed had it not been that the fractured limb has been very much disturbed owing to his having taken a dose of castor oil, his bowels having been confined, and he being obliged to move about every

time his bowels are relieved, all which might easily be avoided at a trifling expense by fitting up a few proper beds for fractures.

LONDON MEDICAL SOCIETY.—ERRATUM.

To the Editor of THE LANCET.

SIR,—Your reporter from the London Medical Society has made a great mistake respecting my observations on the treatment of cholera. I did *not* recommend the same treatment in the *second* stage of the English epidemic which was employed in the first stage of the Asiatic; but, on the contrary, maintained that it would be detrimental; because, although the two stages (the collapse) bore a resemblance to each other, the patients were under very different circumstances. In India, the soldiers (who were bled) were Europeans in the prime of life, and not exhausted by diarrhœa, or impoverished in constitution by want and misery. Here the case is, generally speaking, the reverse. The patients are debilitated by scanty and bad food, and exhausted by premonitory diarrhœa. The Indian practice of bleeding in the collapse *here* will not be productive of the same beneficial effects as in India. I question, indeed, whether more harm than good has not been done *in London*, by venesection in the cold or collapse stage of the epidemic; and one thing is certain, that almost all the experienced practitioners in Southwark, Lambeth, and on the right bank of the Thames, have given up bleeding till re-action commences; and then only employ sanguineous depletion with caution, to prevent topical congestion or inflammation.

I am, &c. &c.

JAMES JOHNSON.

27th March, 1832.

CORRESPONDENTS, &c.

The letters forwarded by *Mr. Hoare* shall appear next week.

W. W. W. might attain his object by advertising, if he cannot do so by private treaty. Our publisher, however, would be better able to give him information, when he (W.) is in town.

Messrs. H. and D. The E. I. Soujee does not appear to us to be an article particularly demanding recommendation, though we have no objections to make to it.

Mr. W. Wilson, of Dublin, for whose letter we are unable to find space, "recommends, as a very powerful stimulant at the commencement or during the stage of collapse in cholera, a bath composed of ol. terebinth. of from 96 to 98o Fah." or higher, if necessary, to be followed immediately by frictions.

A Constant Reader, (J. S.) No. The apprenticeship to the Edinburgh surgeon will not avail.

We refrain from inserting the letter of *J. M.*, because we believe the sum mentioned is overrated by many times the true amount.

We did not intimate any intention of commenting on the former letter of *Mr. H.*, as he will see on referring to it. It is hardly necessary to add the extract from "Pinnock."

The case forwarded by *Mr. L. T.* presents nothing that is new novel or curious.

We are obliged by the attention of our correspondent *C. E. B.*, but have been compelled to omit his as well as several other communications this week.

Dr. Seymour's clinical lecture, and reports from St. Bartholomew's, Guy's, and St. Thomas's Hospitals, are unavoidably postponed until our next number.

ERRATA.—Autopsy of Dr. Hopkins, p. 875, l. 10, for "marked," read *masked*. Note to Mr. Sharpe's letter, p. 866, l. 8, for "Mickel," read *Meckel*. Transfer the "to," l. 10, to the syllable "most," immediately below it.

END OF VOL. I.—1831-32.

INDEX

TO

VOLUME I.—MDCCCXXXI—XXXII.

A

Abernethy, Mr., portrait of, 337.
Abscess, point essential to the healing of, 928.
Abstinence, efficacy of, in gastritis, 478.
Accident, trifling, fatality of one, 48.
Acids, vegetable, injurious effects of, in typhus, 759.
Acupuncturation, Dr. Elliotson on the uses of, 692.
Adams, Mr., account of an operation of lithotomy, related by, 718.
Address to the readers of the Lancet on commencing a new volume, 1.
Adulteration of medicines, importance of reflecting on, when prescribing, 913.
Age of a patient, modification of doses according to, 909.
Ague, Prof. Elliotson on cases of, 197, 312; Dr. Billing on cases of, 237, 328, 430, 582.
Aloes, decoct. as a purgative, 583.
Alpha, remarks of, on the malignant cholera, 17, 39, 108, 179.
Alum, cure of toothach by, 611.
Amesbury, Mr., religious warning of, to the London Medical Society, 684, 801.
Amputation of the upper arm at the West. Hosp., 347; plates, review of Maingault's, by Cox, 488; above knee, case of, at Sheffield, 809.
Anasarca during pregnancy, treatment of, 117; Prof. Elliotson on cases of, 133, 311; remarks by Mr. Bain on, 654.
Anastomosis between the fourth and fifth pairs of nerves, 828; supposed discovery of, 866.
Anæmia, with disease of the heart, 589.
Anatomical theatre, destruction of Mr. Moir's, at Aberdeen, 479; model, Dr. Anzoux's, 703; Plates, review of Lizars', 487; Atlas, review of Weber's, 709.
Anatomy, introductory Lecture to a course of, by Dr. Jacob, 201; review of Cox's Synopsis of, 285; of Bransby Cooper's Lectures on, 286; review of Craigie's Elements of, 406; surgical review of Burt's Illustrations of, 808.
Anatomy Bill, remarks on, and suggestions for the formation of a new one, 397, 451; analysis of Mr. Warburton's, 480; objections to its clauses, 481; proofs of its inefficiency, 486; letter from the College Council on, to Lord Melbourne, 512; address to medical students on the subject of, 520; letter from a country anatomist on the inefficiency of, 521; important information relative to, 572; attempted second reading of, and insuperable objections to, 593; progress and probable rejection of by the Lords, 625; discussion on at the West. Med. Soc., 664; continued opposition to, 707; petition from Leeds against, 708; further objections to, and examination of arguments in favour of, 930.
Anchylosis, pure, general, case of, 27.
Aneurism femoral, case of, in which the external iliac artery was tied, 102; of the external iliac and femoral arteries, 313, 347, 457; spontaneous cure of, by ossification, 429; of the cœliac axis, 458; of the aorta, Mr. Brodie, on supposed cures of, 908; Mr. Brodie on the old treatment of, 928; remarkable fact respecting the blood in cases of, 929.
Antimony, tartrate of, of pustules on the tongue produced by the, 46; use of in typhus, 759; use of in croup, 763.
Antrum, removal of the super maxillary bone, for disease of, 694.
Anus, imperforate, clinical remarks on, fissure of, by Dupuytren, 838.
Aorta, Mr. Brodie on supposed cures of aneurism of, 908; cases of disease of the, 590, 754.
Aphonia, Prof. Elliotson on cases of, 133, 312.
Apoplexy, Dr. Billing, on cases of, 723.
Apothecaries, attempt of a dub to injure, 101.
Apothecaries Company, of London, legal

necessity for passing, 12; scandalous regulations of, 154; of Dublin, injustice of, to medical students, 748.
Apprenticeships, effect of the College regulations in diminishing the premiums for,
Arsenic, new test for the oxide of, 97.
Artery, femoral, supposed rupture of, 134; operation for aneurism of, 103; external iliac, operation for aneurism of, 313, 347, 457; unique variety of, 730; radical proposal to open, in cholera, 744.
Ascites, case of, with bellows sound in the heart, 587.
Asphyxia, Mr. J. Thomas on, 23; application of cupping-glasses in, 167.
Atmospherical commotions, alleged production of cholera by, 113.
Asthma, efficacy of the "lobelia inflata" in, 434; dry, Dr. Billing on a case of, 504.
Aulsebrook, Mr. G., account of a late epidemic in Staffordshire by, 217.
Auzoux, Dr., anatomical model by, 703.
Auscultation in diseases of the heart, cautions respecting, 587; value of, in diseases of the lungs, 925.

B

Badeley, Mr. J. C., Latin epigram by, and English version of, 604, 843.
Bain, Mr., remarks by, on anasarca, 654.
Barnett, Mr. T. W., case of cholera by, 766.
Bats, rare whack at, showing their character, and pestilential influence on medical science, 1; their mode of congregating in clubs, &c., 4; their falling condition, 5; farther remarks on, 51; Dr. James Johnson's criticisms on, 96; remarks on the enormities of, 333; existence of, at Worcester, 335; conduct of, towards other surgeons, 903.
Bell on cholera, remarks on, 110.
Bell, Sir Chas., conduct of, at the Lon. Univ., 842.
Bellows sound, in affections of the heart, Dr. Elliotson on the, 587, 591; cause of, 755.
Billard, Dr. C., extraordinary case of blue perspiration by, 105.
Billing, Dr., Clinical Lectures by:—Introductory Clinical Lecture by, at the London Hospital, 223; demonstration of a diseased heart by, 236; on cases of ague, 237, 328, 423, 424, 430, 583; on spasmodic cholera, and its similarity to the cold stage of ague, 238; on three cases of fever, 329; on a case of pleuro-peripneumony, 330, 424, 583; on fever complicated with peripneumony, 331, 425, 583; on cases of leucorrhœa, hysteria in man, chlorotis hysteritis, 425, 427, 502, 583; hepatised lung, 428; on a case of aneurism cured by ossification and contraction of the mitral valve, 429; on sciatica cured by moxa, 431, 583; on a severe case of hysterical epilepsy, 502, 583; on a case of chlorosis, 503, 583; on dry asthma, 504; on acute pulmonary catarrh, 505; on erysipelas phlyctænoides and carbuncle, 583; on erysipelas of the knee and leg, 585; on three cases of rheumatism, 585; on cases of synocha petechialis or Sydenham's "new fever," 645, 721; on the contagiousness of new fever, 649; on apoplexy and paralysis, 723; letter from, on the treatment of cholera, 652; observations by, on his views of inflammation, 830.
Birtwhistle, Messrs. W. and J., cases of malignant cholera by, 160, 866, 917; cases of dysentery by, 822.
Bishop, the Burker, phrenological developments of, 532, 605.
Blair, Mr. C. E., cases of cholera by, 765, 825.
Blandin, M., Burt's Illustrations from, review of, 808.
Bleeding, effects of, in croup, 761, 836, 874.
Blicke, Dr., letter from, to Messrs. Key and Tyrrell, 858; editorial remarks on the action against, 862, 903.
Blood, correspondence relative to Dr. Stevens's experiments on the, in fever, 376, 591, 657; experiments on, by Dr. O'Shaughnessy, in cholera, 490, 929; analysis of, in cholera, by Dr. Clanny, 501; experiments of Mr. J. Lyon on, 655; aneurismal, invariable inflammation of, 929.
Board of Health, remarks on the precautions put forth by, 156; public notification of, 158; establishment of one at Camberwell, 227; dismissal of Sir Henry Halford from, 304; formation of one at Liverpool, 305; Central, commencement of, 304; establishment of the Cholera Gazette by, 594; announcement of cholera by, in London, 741; charged with being too reserved, 771; payments to the members of, 778; communications of with Mr. Sheldrake, 898; see also *Cholera*.
Body-snatching, origin of, 835.
Books proper for students, 63.
Botany, neglect of, by pharmacopolists, 917.
Bowels, expulsion of, per anum, 821; case of severe hæmorrhage from, 829.
Boyle, Medical History of the Coast of Africa, review of, 487.
Brain, the, operations of, during dreaming, 919.
Breathing, impediments to, in the horse, 781.
Bristol, operation of lithotrity at, 843.

British Lying-in Hospital, objections to the admission of male students at, 443.
Brodie, Mr., clinical lecture by, at St. George's Hospital, 693; amputation of the leg by, 694; on a case of fistula in ano, 907; on supposed cures of aneurism of the aorta and disease of the nasal bones, 908; on the ligature of hemorrhoids, 927; on an hysterical affection of the knee, 928; on gristly, indurated strictures, 928; the nature of hospital gangrene, 928; a point essential to the healing of an abscess, 928; the cure of aneurism, 928; some precautions in dividing the sphincter ani, 928; peculiarity in aneurismal blood, 929.
Bronchitis, Professor Elliotson on a case of, 324.
Bronchocele in animals, 749.
Bronchotomy in asphyxia, 23.
Buckland, Mr. J. P., on the chloride of sulphur in cutaneous affections, 46.
Bullen, Mr. S., note from, 523.
Burkers, disposal of the bodies of, 404; remarks by Dr. J. G. Smith on the late murders by, 439.
Burking, Dr. P. H. Green's remedy for, 521; best remedy for, 219; the effect of on dissections, 340; editor's remarks on the law necessary to check the practice of, 373, 397.
Burne, Dr., extraction of a fork from a sailor's back by, 662.
Burnett, Sir W. remarks on the proposed present to, 43, 44, 45.
Burrows, Dr., remarks on a lecture of, 832.
Burt's Illustrations of Surgical Anatomy, review of, 808.

C

Calculi, discharge of, from the abdomen, 411.
Calculus in the bladder, Mr. Syme's cases of, 35; operation for, by Mr. Lynn, 135; unsuccessful operation of lithotrity for, 208; fatal case of, at the Westminster Hospital, 313; cure of by lithotomy, 618; cure of by lithotrity, 619; case of at the London Hospital, 748.
Calomel, use of in fever, 759; in dysentery, 822; in croup, 761, 836, 874.
Cancer of the breast in a male, 490; of the lips, 939.
Candlish, Mr., charge of, against Dr. Clanny, 668, 844.
Cannibalism, paper on, 488.
Carbuncle, its identity with erysipelas, 584.
Carlisle, Sir A., college scene with, 9.
Catechism of Phrenology, review of, 709.
Cattle medicines, best form for, 576.
Cerebellum, on the sexual function of, 49, 51, 98, 99, 161.
Chapman's Atlas of Surgical Apparatus, review of, 906.
Chloride of sulphur, external application of, in cutaneous affections, 46.
Chlorine gas, reputed disinfecting agency of, shown to be erroneous, 595.
Chlorosis, Dr. Billing on a case of, 593, 583.
Cholera, the malignant, Letters by Alpha against the contagious nature of, 17, 39, 108, 179; Mr. Curtis on the employment of prussic acid in, 47, 117; Dr. Forster on the atmospherical commotions which have attended, 113, 232; Mr. Gower's severe case of, 121; arrival of at Hamburgh, 124; editorial remarks on its probable irruption in England, 125; objections to the doctrine of contagion in, 153; first notification issued by the Board of Health, anticipating the appearance of in England, with editorial remarks, 150, 159; English military cordons against, 157; advice to parishes on the approach of, 157; severe cases of, cured, by Mr. Birtwhistle, 160; advantages of hot-air baths in, 160; on the employment of general cupping in, 167; review of Mr. Kennedy's work on, 175; remedy employed in, by the Jews of Wiesnig, 216; Dr. Jeucken's mode of treating, 216; arrival of, at Sunderland, 221; precautions against, in London, 228; history of the rise, progress, and ravages of, from 1817 to its arrival in England, including editorial remarks on its general character, the term "cholera," its symptoms, its disregard of season and climate, its identity in Russia and Asia, its pathological phenomena, its physiology, chemical analyses of the evacuations in, its remote cause, the controversy regarding its origin, the various theories respecting the contagious character of, methods of treating, and precautions against the spreading of, with advice to the people of England, 241 to 284 (with a map); discussion on the mode of its spreading, 299; editorial remarks on the fall of Sir Henry Halford's Board of Health, and the institution of a new Board to superintend, 304; editorial remarks on proceedings respecting, at Liverpool, 305; editorial cautions on the spread of, 309; review of Forster on, 320; review of Searle and Ewertz on, 323; progress of, in Sunderland, 337; Dr. Johnson's opinions of the contagious nature of, 339, 341; discussion on the treatment and character of, 343; on inhalation of oxygen in,

364; on opening the radial artery in, to obtain blood, 365, 744; proposal to inject highly-oxygenised salts into the veins in, 366; discussion on Mr. Searle's treatment of, &c., 371; editorial cautions against quackery in, 376; remarks on the government announcement of, 377; letter from Sunderland respecting its character, treatment, and progress in that place, 401; appearance of in Newcastle and North Shields, 404; review of Mantell on, 407; discussion on the nature and treatment of, 407; Dr. Barry's opinions on, 408; paper by Dr. Wilson Philip on, and discussion on the characters and treatment of, 444; discussion on Mr. Searle's mode of treating, 446; remarks on the official reports of, 459; proposal to transfuse blood in, 486; Dr. O'Shaughnessy's experiments on the blood in, 490; Mr. Sheldrake's mode of circulating the blood in, 491; case of, at Sunderland, with Dr. Clanny's analysis of the blood, 505; editorial remarks on the inutility of cordons sanitaire, 509; on the liability of the poor to become the victims of, 518; necessity for publication of the treatment of, 524; case of, occurring in Southwark, in October 1831, 539, 767; reports of thirteen cases of, in the north of England, communicated to the Central Board of Health, 543, 745; discussion on the premonitory diarrhœa and consecutive fever of, 569; administration of mustard emetics in, 570; prostrating effect of the atmosphere in, 572; mode of relieving cramps in, 582; editorial remarks on the publication of the Cholera Gazette, 594; proposition for adjusting quarantine regulations in relation to, 595; editorial remarks on the uselessness of chlorine gas as a disinfecting agent in, 596; isolated case of at Doncaster, 600; analysis of Dr. Henry's paper on the disinfecting powers of heat in, 601; Latin verses on the mania for writing on, 604; and translation, 843; case of at Knightsbridge, 620, 696, 800; editorial remarks on a supposed case of at Shadwell, 621; reply to, 712; alleged first appearance of, on the coast of England, 630, 843; Dr. Clanny's remarks on the progress of at Sunderland, 633, 731, 811, 869; Dr. Billing's clinical remarks on the connexion of, with Sydenham's "new fever," 645, 721; and on the contagious nature of, 649; Dr. Billing's remarks on the treatment of, 652; Dr. Clanny's mode of treating the various stages of, 653; history of the progress of, in the north of England and Scotland, including editorial remarks on the mode of its extension, the symptoms which have characterised it in England, its chemical pathology, its treatment, and the sanative measures to be observed in England, 669 to 684; on the efficacy of bark in, 696, 745, 800; editorial remarks on the approach of, to London, and on the evil effect of secrecy in, 706; synopsis of the symptoms and treatment of, drawn up by the medical board of St. Mary's, 713; editorial remarks on the appearance of, in London, and the attempts to deny it, 734; cautions to the public on, 735; cases of, in Southwark, Limehouse, and Rotherhithe, 739; announcement of, in London, by the Central Board of Health, 741; discussion on the appearance of, in London, and on Florance Sullivan's case, 742; relief of the spasms in, 745; recovered cases of, in the Borough, 765; fatal case of, at Limehouse, 766; remarks on the appearance of, in London, 767; review of the discussions on, with remarks on the novelty of, the reserve of the Central Board of Health in, and the charges made against medical men respecting, 768; progress of in London, including editorial remarks on the proper mode of arguing the various questions of; references to undoubted cases of; remarks on the composition of the party arguing against the existence of, and their arguments; the emoluments of the Central Board of Health; and on the question of contagion, and the existing quarantine restrictions, 740 to 780; remarks by Dr. Knight on saline treatment in, 793; Mr. Dodd on the characters of in England, and its treatment at Houghton-le-Spring, 795; Dr. Penneck on the nature and treatment of, 798; Mr. Thomas's synopsis of the treatment of, 799; discussion on the existence of in London, 802; exposure of the poor to, 804; editorial remarks on the denial of in London, the insinuations against medical men, and the payments to the Central Board of Health, 805; official regulations respecting, directed to medical men, with editorial remarks on the duties of the profession, 806; observations of Dr. Green on the identity of, in London and Asia, 824; fatal case of, in Southwark, 825; peculiar exposure of the poor to, 826; discussion on the existence of in London, 834; formation of an association to investigate, with editorial remarks on its inutility, 844, 864; case of in the Mile End Road, 866; proofs that it is a new disease in England, 867; discussion on various points connecting it with the cho-

lera of Leeds in 1825, 871; the alleged discovery of Delpech in the pathology of, shown to be erroneous, 884; editorial remarks on the alleged discovery of Delpech in, 905; case of at Vauxhall, 885; Mr. Dobson's account of the epidemic cholera in Leeds in 1825, in contrast with, and discussion on, 894; discussion on the identity, contagious nature, and subsequent fever of, 896; communications of the Central Board with Mr. Sheldrake respecting the treatment of, 898; address of the association for inquiring into, 902; fatal case of in a previously robust child, 917; attempt to explain the pathology and deduce the treatment of, 918; temperature of the bodies of patients affected with, 919; non-remuneration of surgeons attending the hospitals for cases of, 927; review of O'Shaughnessy on the Chemical Pathology of, 929; complete establishment of the identity between that of Russia, Poland, Prussia, the north of England, and London, 935; summary (with remarks) of the cases and deaths in England and Scotland occasioned by, 938.

Christison, Dr., on the effects of opium eating, 614.

Cinchona bark, efficacy of in cholera, 696.

Cinchonine, mode of making tasteless, 838.

Clanny, Dr., analysis of blood taken from cholera patients by, 501; additional letters from, 633, 731, 811, 869; on the treatment of cholera, 653; charge of Mr. Candlish against, 668, 844.

Clarke, Mr. J., on the effect of fear in predisposing to disease, 914.

Climate, differences in the effect of medicines according to, 911.

Cock, Dr., history of the scarlet fever at St. Bartholomew's Island by, 515.

Coleridge the poet, phrenological developments of, 921.

Cœliac axis, case of aneurism of, 457.

Colchicum, Dr. Elliotson on the important properties of, 690.

Coley, Mr. J. M. successful performance of the rhinoplastic operation by, 474.

Cold, treatment of asphyxia from, 26.

Colic from lead, Professor Elliotson on a case of, 312.

College of Surgeons in London examination, scene at, and exposure of the whole system of, 9; effect of the regulations at, in diminishing apprenticeship premiums, 92; sale of the lists at, 95; attempts by the Council of, to aid surgical reform, 122; remarks on the exactions of the Council of, 154; attempts of the Council of, to prosecute the editor, 190; new by-laws at, 192; abandonment of the prosecution of the editor by the Council of, 219; letter on the conduct of the Council of, 220; letter from Mr. Dermott to the Council of, 340; editor's remarks on the monopoly of the Council of, in connexion with anatomy, 373; disposal of the bodies of Bishop and Williams by the Council of, 404; worthlessness of the diploma sold at, 442; sly conduct of the Council of, 456; confession of the Council of, to Lord Melbourne, with remarks, 509, 512; examination of the charter under which the Council of, encouraged burkings, 565.

College of Physicians, conversazione at, 898.

Colleges, Medical and Surgical, editor's account of those in London, 5; remarks on expulsion from, 29.

Colvan, Dr. J., case of imperforate hymen by, 695.

Colon, Mr. Salmon's case of stricture in the, 881.

Comets, alleged connexion of with cholera, 114.

Complin, Mr. E. T., letter from, respecting the cholera, 216.

Confectionary, old mode of colouring, 843.

Consultations, medical, new mode of arranging, 832.

Cooper, Professor S., introductory address by, to a course on surgery at the London University, 56; Sir. A., charge of Mr. J. Cornish against, 239; notice of the portrait of, 337; Bransby, review of Lectures on Anatomy by, 285; Mr. J. T., method by, of detecting opium, 712.

Corbett, Dr. R., case by, of a copper coin in the stomach three months, 294; case by, of a pin passing through the intestinal canal, 295.

Corbin, Mr. R., case of lithotomy related by, 618.

Cordons, absurd proposal to establish, in England, 509.

Cornish, Mr. James, charge of, against Sir A. Cooper, 239.

Coroners (*see inquests*).

Costello, Mr., paper by, on the rhinoplastic operation, 699; operation of lithotrity by, at Bristol, 843.

Costiveness, severe case of, 326.

Courtney, Mr. A., on the present to Sir William Burnett, 45; on the efficacy of digitalis and polypodium in the treatment of epilepsy, 45; bequest of the body of, to the London College of Medicine, 667.

Cow-pox, Dr. Howison on vaccination

for, 541; identity of, with small-pox, established, 542.
Cox's Synopsis of the Bones, &c., review of, 285; his edition of Maingault's Amputation Plates, review of, 488.
Craigie's Elements of Anatomy, review of, 406.
Cramps in cholera, relief of, 582, 745.
Crepitous rattle, Prof. Elliotson on the cause of, 923.
Cretinism in dogs, nature and treatment of, 750.
Croup, "lymphatic," Mr. H. Rees on the nature and treatment of, 761; Mr. Whittaker's remarks on, 836; Mr. Rees's rejoinder, 874.
Cupper, remarks on the election of one at the Middlesex Hospital, 100.
Curtis, Mr. J., on the efficacy of hydrocyanic acid in vomiting, 47; reply of Mr. E. Waring to, 117.
Cutaneous diseases, Prof. Elliotson's simplification of the arrangement of, 850.
Cyanopathia cutanea, Dr. Billard's case of, 105.
Cyanuret of potassium, cases of tic douloureux cured by, 873.
Cyclopædia of Practical Medicine, No. I, notice of, 603; alleged defects in an article in, 654.

D

Davis, Dr. D., notice of his work on Obstetric Medicine, 628.
Deafness, effect of galvanism on, 87.
Deal, coroner's inquest at, 158.
Defoe's History of the Plague, quotations from, 777.
Delpech, Prof., case of prolapsus of the tongue cured by compression by, 489; letter of Dr. Green on the supposed discovery of, as to the cause of the malignant cholera, 881; editorial remarks on the supposed discovery by, 905.
Denbighshire Dispensary, testimonial to medical officers of, 524.
Dermott, Mr. G. D., on the cause of certain effects of hanging, 98; letter from, to the College Council on burking, 340.
Dewhurst, Mr. H. W., on an unfair election of a cupper, 100.
Diarrhœa, Prof. Elliotson on cases of, 501.
Dickson, Mr. T., report by, of a case of expulsion of the bowels, 821.
Digestion, Dr. M'Cormac's theory of, 476; remarks on the theory of, 697.
Digitalis, Mr. Courtney on the efficacy of, in epilepsy, 45; common practice of giving, in Ireland, 98; note relating to the use of, 442; Dr. Sharkey on the efficacy of, in epilepsy, 650; Dr. Wood on the efficacy of, in phthisis, 915.
Dinner to the London University professors, 812.
Disease, effect of fear in predisposing to, 914.
Dobson, Mr. W., case of ovarian disease by, 89.
Dodd, Mr. H., on the cholera at Houghton-le-Spring, 795.
Dodgson, Mr. J., discovery of a new test for the oxide of arsenic by, 97.
Dog, anatomy and diseases of (*see Youatt, Mr.*)
Doncaster, case of cholera at, 600.
Doses of medicines, Gaubrus table for the regulation of, 999.
Dreaming, paper on, the connexion of with the doctrines of phrenology, 949.
Drinks, acidulated, injurious effects of in typhus, 750.
Dropsy, universal, Prof. Elliotson on a case of, 198.
Drowning, pathology and treatment of, 23.
Druggists and physicians, on the connexion between, 100.
Dublin College of Surgeons, introductory lecture to Prof. Jacob's course of anatomy at, 201; Apothecaries Company, injustice of, to medical students, 748.
Dupuytren, M., fissure in ano treated by, 838.
Dysentery, chronic, Prof. Elliotson on cases of, 395, 469; severe, cases of, treated with calomel, 822.
Dysmenorrhagia, sketch of a case of, 760.

E

Ecthyma, Dr. Elliotson on a case of, 853.
Eczema, Dr. Elliotson on a case of, 852.
Edinburgh Medical Journal for Oct. and Jan., reviews of, 33, 514.
Edwards, Mr. D. O., introductory address of, to a course of clinical demonstrations, 143; notices respecting the discontinuance of, 332, 444.
Electricity, effect of, on deafness, 87.
Elliott, Mr., successful case of lithotrity communicated by, 619.
Elliotson, Prof., introductory lecture by, to a course on the principles and practice of physic, delivered at the London University, 64; Clinical Lectures by, on cases occurring at St. Thomas's Hospital:—On cases of injury to the spine, chronic gastritis, and diseased heart, 132; anasarca, general paralysis, aphonia, and phthisis, 133; case of hydrophobia, showing the inefficacy

of the carbonate of iron in, 162; cases of injury to the spine, disease of the liver and spleen, chronic gastritis, and hysteria, 196; cases of ague and continued fever, 197; cases of universal dropsy, 198; cases of chronic gastritis, 309; a case of diseased heart, 310; cases of ana-asca, with scrofula and hysteria, 311; cases of imposition—colic from lead, ague, and aphonia, 312; on bronchitis and hydrocephalus, 321; case of severe constipation, 326; two cases of fever, 327; rheumatism and itch, 328; on worms in the alimentary canal, 390; on St. Vitus's dance in boys, 392; a case of continued fever, 393; a case of jaundice, 394; a case of chronic dysentery, 395; a case of rheumatism and disease of the heart, 396; on urticaria and its causes, 432; a case of rheumatic neuralgia, 433; asthma, and the efficacy of the lobelia inflata in, 434; cases of diseased heart, chronic gastritis, and inflammatory headach, 435; on cases of chronic dysentery and diarrhœa, 469; the treatment of chronic gastritis, and on a case of hypochondriasis in a male, 473; on a case of syphilitic lepra, 498; on the communication of syphilis to the fœtus; a case of phthisis in a child, 500; an interesting case of disease of the heart, with ague, and intermitting bloody urine, and cases of rheumatism and diarrhœa, 501; diseases of the heart and their treatment, with cases, 486; erythema and its varieties, 689; the properties of colchicum, 690; a case of erythema tuberculatum and lumbago, 691; the efficacy of acupuncture in rheumatism, 692; a case of fever admitting of unusual administration of wine, with a trial of saline agents, 725; late changes in the character of fever, 726; a case of scirrhus uteri, treated by the potass. hydriod., 727; a case of hemiplegia, with remarks on paralysis, 729; diseases of the heart, 754, 823; a case of enlargement of the liver, with jaundice, 756; a simple mode of arranging diseases of the skin, 850; cases of impetigo, eczema, rupia, ecthyma, and erysipelas, 851, 854; instructive case of diseased lungs, with remarks on the cause of the crepitous rattle and pectoriloquy, 923; notes from the reporter of the clinical lectures of, 199, 298; paper by, in defence of phrenology, 287, 357; paper by, on the phrenological developments of Bishop and Williams, 532, 605.

Emetics, their immediate mode of operating, 909.

Ensor, Mr. S. R., case of spasmodic cholera by, 620, 800.

Epidemic, extraordinary in Staffordshire, account of, 217.

Epilepsy, Mr. A. Courtney on the efficacy of digitalis and polypodium in, 45; common practice of giving digitalis for the cure of, in Ireland, 95; letter of "Calus" relating to, 442; Dr. Billing on a severe case of, 502, 509, 583; Dr. Sharkey on the efficacy of digitalis in the treatment of, 650.

Erysipelas, use of mercurial ointment in, 32; efficacy of bleeding in, 76; phlyctænoides, interesting case of, 583; of the knee and leg, 585; Dr. Elliotson on three cases of, 853.

Erythema and its varieties, Prof. Elliotson on, 689.

Etheldreda, Queen, stealing of the body of, 835.

Evans, Mr. G., case by, of union of a one-lobed lung to the pleura costalis, 438.

Eunuch, attempts of one to produce sexual pleasure, 49.

Ewertz on Cholera, review of, 323.

Eye, fatal case of wound of, by a tobacco pipe, 715.

F

Fear, effect of, as an exciting cause of disease, 914.

Fearn, Mr. S. W., supposed discovery by, of an anastomosis between the fourth and fifth pair of nerves, 828.

Ferguson, Dr., letter from, relative to Dr. W. Philip's pamphlet, 711.

Fever, Mr. S. Gower on the propriety of bleeding in puerpural, 120; continued, Dr. Williams's views of the nature and treatment of, 183, 204; Prof. Elliotson on various cases of, 197, 327, 393, 432, 725; simple, Dr. Billing on three cases of, 329, 331; with pleuro peripneumony, Dr. Billing on cases of, 425, 583; Dr. Stevens on the efficacy of saline agents in (*see Stevens*); late important change in the character of, 572; Sydenham's *new* cases of, Dr. Billing on, 615, 721; remarks by Prof. Elliotson on the late changes in the character of, 726; in cattle, temperature in, 575; Dr. Knight's proximate cause of, on the, 709.

Fistula, recto-vaginal, cases of, 35.

Fistula in ano, Mr. R. Lanyon's case of, 153; in the abdomen, Dr. Stokes's case of, 612; case of, treated by Dupuytren, 538; Mr. Brodie on a case of, 907.

Fœtus, Mr. Gower on malformation of, 117; growth of, after great compression of the brain, 119.

Forster, Dr. T., on the atmospherical commotions which have accompanied

the malignant cholera, 103, 232; on cholera, review of, 320.
Fox, Mr. L. O., case of fracture of the skull related by, 48; case of diseased heart following acute rheumatism, by, 823.
Fracture, rare case of, with remarks, 33; on the position of broken extremities in, 33; vesications from external injury accompanying, 33; danger of too early motion after, 34; of the skull, case of, 48; of the os pubis, with injury to the bladder and rectum, 115.
Fracture beds, want of, at the London Hospital, 939.
Friction, efficacy of, in cholera, 491.
Fungus hæmatodes of the hip, operation for, 939.

G

Galvanism, effect of, on deafness, 87.
Gane, Mr. J., case of spasmodic cholera by, 539; remarks of, on the appearance of cholera in London, 767.
Gangrene, supposed, in fractures, 33; of hospitals, Mr. Brodie on the nature of, 928.
Gastric juice, remarks on, 697.
Gastric irritation, causes, effects, and treatment of, 758.
Gastritis, chronic, Professor Elliotson on cases of, 131, 196, 309, 435, 473; necessity of abstinence in, 478.
Genital organs, effect of excitement of the cerebellum on, 161, 661.
Girthing, tight, evil of, 818.
Glanders in the human subject, denial of, 99; in the horse, 461, 492, 525; similarity of ozena to, 573; fatal case of, in man at Dublin, 698.
Glasgow, expense of medical education at the schools of, 129.
Goring, Dr., letter from, on the cholera at Sunderland, 377; on friction with tincture of opium in the spasms of cholera, 745.
Gower, Mr. S., professional reminiscences of, 117.
Grant, Prof., defence of, against the charges of Mr. Pattison, 199.
Green, Dr. P. H., proposed remedy for burking by, 522; observations by, on a unique variety of the femoral artery, 730; on the identity of the cholera in London with that of Asia and Russia, 824; letter from, showing the supposed discovery of Prof. Delpech in cholera to be erroneous, 884.
Gregory, Dr., on the absence of albumenous urine in diseased kidney, 36; case by, of infiltration of the lungs with coal dust, 38; chemical investigations of, into renal disease, 517.
Guthrie, Mr., examination of a pupil by, 9; operation of tying ligature of the external iliac artery by, 103, 313.
Guy's Hospital, medical schools near, 15*.

H

Hæmorrhage, death of a mother and child from, the umbilical chord not having been tied, 339; secondary, after amputation at the Sheffield Hospital, 809; from the bowels, 829.
Halford, Sir H., and the Board of Health, 304; political letters of, 490.
Hall, Mr. J., paper by, on the first appearance of the epidemic cholera on the coasts of England, 630.
Halma Grande, M., supposed discovery of a new styptic by, 522.
Hamilton, Mr. W., case by, of fistula in the abdomen, 612.
Hand, severe injury of, 939.
Hanging, letters on the effect of, on the sexual organs, 49, 51, 98, 99, 161.
Hardwicke, Mr. A., on the physiology and treatment of the malignant cholera, 918.
Hardy, Mr., on menstruation from hanging, 51, 99.
Hargrave's Manual of Surgery, review of, 625.
Harling, Mr. G., note from, 51.
Hawkins, Dr. Bisset, remarks on his work on the Asiatic Cholera, 39, 108.
Head, review of a Treatise on Injuries of, 405.
Headach, inflammatory, Prof. Elliotson on a case of, 435.
Heart, Dr. Paget on malformations of the, 36; Prof. Elliotson on cases of disease of, 132, 310, 396, 435; diseased, Dr. Billing's demonstration of one, 236; diseased, Prof. Elliotson on an interesting case of, with ague and intermitting bloody urine, 501; general remarks on affections of, and their treatment, 586; case illustrating the diseases of, 588, 589, 590, 754; Mr. Fox's case of disease of, after acute rheumatism, 823; disease of, in Dr. Hopkins, 839, 875.
Heat in animals, how to ascertain, 575; Dr. Henry's experiments on the disinfecting powers of, 600; claims of Mr. Wallace, of Dublin, to the discovery of the disinfecting powers of, 854; effect of the application of, in some forms of disease, 916; in cholera patients, remarks on, 919.
Hemiplegia, Prof. Elliotson on a case of, 729.
Hemorrhoids, Mr. Brodie on the ligature of, 927.

Henry, Dr., experiments by, on the disinfecting powers of heat, 600.
Hermann, M., fallacious experiments of, on the blood in cholera, 930.
Hernia, vaginal, Mr. Taylor's case of, 205; enormous congenital umbilical, case of, 666.
Heron, Mr. G., case by, of hæmorrhage from the bowels, 829.
Heurteloup on Lithotrity, review of, 169; letter from, 199; operations of lithotrity by, 619, 830.
Hiley, Mr. S., on the sulphate of indigo as a test for nitric acid, 886.
Hoare, Mr. J., account by, of a person who would not speak to males, 441.
Holmes, Mr. H., letter from, 745.
Hooper, Mr., charge against, and reply of, 842, 875.
Hopkins, Dr., death of, 839, 875.
Horse, Mr. Mogford's cases of enlarged spleen in, 872; anatomy and diseases of (*see Youatt, Mr.*)
Horsenell *v.* Blicke, Dr. Blicke's remarks on the evidence of Messrs. Key and Tyrrell at the trial of, 858; editorial remarks on the trial of, 861.
Hospital abuses, petition against, 524; autopsies, neglect of, 905; want of one at the London University, 66; decision of the Council to build one, 841; at Sheffield, amputation at, followed by secondary hæmorrhage, 809.
Hospitals in London, list of, 15.
Hot-air baths, notice of Jones's and Rippon's, 907.
Houghton-le-Spring, Mr. Dodd on the treatment of the malignant cholera at, 795.
Howison, Dr., remarks by, on vaccination, 540; on some important considerations to be attended to in prescribing medicines, 909.
Hudson, Mr., remarks by, on the preparations of hydrarg. c. creta, 189.
Hulke, Mr. W., jun., letter from, on an inquest at Deal, 158.
Hunterian Medical Society of Edinburgh, discussions at, 410.
Hunterian oration for 1832, 705.
Hydatids, formation of, in the liver, 767.
Hydrargyrus c. creta, remarks on the preparation of, 189.
Hydriodate of potash, Lugol on the use of, in scrofulous diseases, review of, 76; trial of, in scirrhus uteri, 727; safety of large doses of, 728; remarks on Prof. Elliotson's employment of, 742; employment of, in phthisis, 764.
Hydrocele, operation for, 939.
Hydrocephalus, Prof. Elliotson on a case of, 324.
Hydrocyanic acid, Mr. Curtis on the suppression of vomiting by, 47.
Hydrophobia, Mr. Simpson's case of, 29; trial of the carbonate of iron in, 162.
Hymen, imperforate case of, 695.
Hypochondriasis, Prof. Elliotson on a case of, 473.
Hysteria, Prof. Elliotson on cases of, 196, 311; Dr. Billing on cases of, 425, 427, 502.

I

Idiosyncracy of patients in connexion with the doses of medicines, 910.
Impetigo, Prof. Elliotson on a case of, 851.
Indigo, sulphate of, as a test for nitric acid, 886.
Infanticide, suspicious case of, 339.
Inflammation, Observations of Dr. Billing on, 830.
Inhaling, instrument for compressing the nose during, 101.
Innominata, cases of disease of, 591, 754.
Inquest, coroner's, at Deal, account of one, 158; at Lisson Grove, 297; dangerous idea propagated at one, 339; at Shadwell, in a suspected case of malignant cholera—letter from Mr. Mason, 712; at Liverpool, remarks on one, 746; in the King's Bench prison, 842, 875.
Insignia, professional, remarks on, 260.
Iodine in scrofula, Lugol on, review of, O'Shaughnessy's translation of, 73; medicinal effects of, on animals, 750.
Iron, carbonate of, trial of, in a case of hydrophobia, 162; efficacy of, in acemia, 589.

J

Jackson, Dr. G., case of melena by, successfully treated with turpentine, 818.
Jacob, Prof., introductory lecture to a course of anatomy by, 201.
James, Mr. G. W., case by, of viper bite, 294.
Jaundice, Prof. Elliotson on a case of, 394; with enlarged liver, case of, 756.
Johnson, Dr. James, remarks by, on hospital surgeons and physicians, 96; letter from on the non-contagious nature of cholera, 338; note from, 376; reply of, to Dr. Stevens, 591; rejoinder of Dr. Stevens to, 657; remarks on the views of, relative to the malignant cholera, 778; note from, relative to some remarks by, 940.
Jones's hot-air baths, notice of, 907.
Jowett, Mr. R., obituary of, 95.

K

Kennedy on the Contagious Cholera, review of, 173.

Key, Mr., letter of Dr. Blicke to, 853.
Kidney, diseased, Dr. Gregory on the connexion of, with abdominal urine, 36; chemical investigations into disease of, 517.
King's Bench prison, inquest in, 842.
King's College, expenses of medical education at, 155; enmity of Messrs. Mayo and Partridge to phrenological demonstrations at, 519.
Kiver, Dr., on the treatment of cholera, 216.
Knee, hysterical affection of, 928.
Knight, Dr. P. S., on the use of alkaline salts in cholera asphyxia and typhus fever, 758, 785.
Knox, Mr. G. F., notes from, 441, 875.
Kruger, Mr. J., account by, of an unsuccessful operation of lithotrity, 208.

L

Labour, obstructed, Mr. Tomkins's case of, 747.
Lancet, The, prefatory address of the Editor to the readers of, 1; effect on Dr. Elliotson's practice, of publishing the clinical lectures of, in, 70.
Lanyon, Mr. R., case of menorrhagia rubia by, successfully treated with the ergot of rye, 115; case of fistula by, 153.
Larynx, the, double in birds, 754.
Leary, Mr., suggestions of, for treating cholera by transfusion of blood, 486.
Lecanu, M., analysis of the blood by, 931.
Lecturers, list of, at the London Medical schools, 13.
Lecturing, remarks on the practice of, 56, 584.
Leeds, petition from, against the Anatomy Bill, 708.
Leg, nervous tubercle of, 36.
Lepra syphilitica, Prof. Elliotson on a case of, 498.
Leucorrhœa, Dr. Billing on a case of, 425, 430, 502, 583.
Levinge, Mr. Thos., obituary of, 231.
Levison, Mr. J. L., on the sexual function of the cerebellum, 49.
Lightning, treatment of asphyxia from, 26.
Lighthouse, cases of cholera at, 749, 766.
Lithotomy, fatal case of, 34; operation of, by Mr. B. Lynn, 135; Mr. R. Corbin's case of, 618; operation of, at the London Hospital, 748.
Lithotrity, review of Heurteloup's Treatise on, 169; unsuccessful case of, 208; case of stone in training for, at the Westminster Hospital, 230; cases of, performed by Baron Heurteloup, 619, 830; performed by Mr. Costello at Bristol, 843.
Liver, Prof. Elliotson on a case of, 196; case of enlargement of, with jaundice, 756.
Liverpool, scandalous coroner's inquest at, 746.
Lizars' anatomical plates, review of, 487.
London College of Medicine, remarks on the Eleemosynary Fund of, 29, 31; postponement of the general convocation of, 125; on the power of expulsion from, and admission of junior candidates for the diploma of, 29, 127; letters from candidates for the diploma of, 195; prospectus of, 456; Mr. Courtney's bequest of his body to, 667.
London Fever Hospital, important change in the character of fever observed at, 572.
London Hospital, clinical lectures delivered by Dr. Billing on cases occurring at, 233, 328, 423, 429, 502, 582, 645, 721; trial of Messrs. Talrich and Halma Grande's quack styptic at, 522, 620; alleged injustice to medical students at, 593, 636, 666; removal of the superior maxillary bone at, 604; operation of lithotomy at, 748; various cases at, 939.
London Medical Society, first sessional meeting of, 1831-32, 71; discussions on the malignant cholera at, 371, 446, 569, 630, 743, 768, 834, 897; case of malignant small-pox related at, 629; remarks at, on the use of the red globules of the blood, 630; religious warning of Mr. Amesbury to the members of, 684, 801; observations at, on Dr. Elliotson's doses of the hydriodate of potash, and the adulterations of that medicine, 742; discussion at, on the epidemic cholera of Leeds in 1825, 896; objections to signs of disapprobation at, 901.
Lumbago, Prof. Elliotson on the treatment of, 692.
Lungs, diseased, remarks by Prof. Elliotson on a case of, 123; infiltration of, with coal dust, 38; case of hepatization of, 428; lung, one lobed, union of, to the pleura costalis, 438.
Lynn, Mr. B., operation of lithotomy by, 135.
Lyon, Isaac, experiments of, on the blood, 655.
Lyons, Mr. P., case of pure general anchylosis by, 27.

M

Macmichael, Dr., remarks on the cholera pamphlet by, 17, 39, 108.
Magnesia, carbonate of, in poisoning by sulphuric acid, 436.

Malapraxis, suggestions to medical men on giving evidence in cases of, 876.
Mantell, Mr. G., on the use of mercurial ointment in erysipelas, 32; rules by, for the treatment of cholera, review of, 407.
Mart, Mr. G. R., compress by, for the nostrils during inhalation, 101; on the evil effects of opium eating, 710.
Mason, Mr. S., letter from, 712.
Materia medica, effect produced by various circumstances on the components of, 912.
M'Carthy, Mr., note from, 812.
M'Cormac, Dr., theory of digestion by, 475, 697.
Males, account of a man who would not speak to, 441.
Mayo, Mr., complaint against, at the Middlesex Hospital, 811; conduct of,
Meath Hospital, contrast between the modes of electing clinical clerks at, and the Westminster, 338; case of fistula in the abdomen at, 612.
at the London University, 842.
Medical men, suggestions to, on giving evidence in cases of malapraxis, 876; bad practices of some, 72, 989; education of, expense of, at Glasgow, 129; alleged exorbitant charges made by, 227.
Medical Jurisprudence, review of Ryan on, 137; examinations connected with, an excellent test of professional knowledge, 621.
Medical profession, state of the, in the country, 667.
Medicine, forensic, remarks on, as a general test of professional knowledge, 621.
Medicine and surgery, intimate connexion between, 59.
Medicines, narcotic, primary and secondary effects of, 437; important circumstances to be attended to in prescribing, 909.
Medico-Chirurgical Society, consent of, to the publication of its proceedings, 335.
Melbourne, Lord Viscount, letter of the Council of the College of Surgeons addressed to, 512.
Melena, Dr. G. Jackson's case of, successfully treated by oil of turpentine, 818.
Menorrhagia rubia, Mr. R. Lanyon's case of, 115.
Menstruation produced by strangulation (*see hanging*).
Mercurial ointment in erysipelas, efficacy of, 32; in testitis, 47.
Merry, Mr. R., case of fractured os pubis, with injury to the bladder, &c., 115.
Middlesex Hospital, mis-election of a cupper to, 100; abuses at, 524, 811.
Midwifery, Dr. Nagle on the use of the stethoscope in the practice of, 447.
Milburne, Mr., note from on the cholera, 524.
Millington, Mr. T., case by, of expulsion of the placenta previous to the birth of the fœtus, 232.
Mitchell, Mr. S., on Dr. Taylor's proposal to open the radial artery in cholera, 744; Mr. J. T., case of malignant cholera by, 885.
Mogford, Mr. W., cases by, of enlarged spleen in the horse, 872.
Moir, Mr., destruction of his theatre of anatomy at Aberdeen, 479.
Monoclepsis, Mr. Reed's case of, 167.
Monks the first body-snatchers, 835.
Morphine and its salts, test of, 838.
Moss, Mr. W., on the efficacy of mercurial ointment in testitis, 47.

N

Nagle, Dr., on the use of the stethoscope in cases of pregnancy, 447.
Narium to be used during inhalation, 101.
National Political Union, reply to the arguments used at, in favour of Mr. Warburton's Anatomy Bill, 930.
Naval surgeons, letters relating to (*see Burnett, Sir W.*)
Navy, present state of the medical profession in, 634; grievances of assistant-surgeons in, 714.
Neale on Animate Contagions, review of, 317.
Negri, Dr., case of malignant small-pox communicated to the London Medical Society by, 629; observations of, on a case of cholera at Knightsbridge, 696, 800.
Neill, Mr. H., case by, of discharge of calculi from the abdomen, 411.
Nerve, facial, Mr. Sherriff's new method of dividing, 632.
Nerves, 4th and 5th pair, supposed discovery of an anastomosis between, 828, 866.
Nervous system, sympathy between the various parts of, 760.
Neuralgia, inefficacy of bleeding in, 761.
Nitric acid, Mr. Hiley on the sulphate of indigo as a test for, 886.
Nose, ulcer of the, 35; interesting case of disease of the bones of, 908; Mr. Costello's paper on a new rhinoplastic operation for the reproduction of a new one, 699.
Nutrition, connexion of the thymus gland with, 784.
Nymphomania, cure of, by wet cloths, 50.

O

Obituary of Mr. Robert Jowett, 95; of Mr. Thomas Levinge, 231; of Dr. E. J. Hopkins, 838, 875.

Obstetric Medicine, Dr. Davis's work on, notice of, 628.
Operation of lithotomy by Mr. Syme, 35; of tying the external iliac artery by Mr. Guthrie, 103, 313; of lithotomy by Mr. B. Lynn, 135; of lithotrity, Mr. James Bruger's unsuccessful case of, 208; of amputation of the upper arm, by Mr. White, 346; of extirpation of the superior maxillary bone by Mr. Earle, 379; for the partial reproduction of a nose, by Mr. Coley, 474; for the extirpation of a large scirrhous tumour by Dr. Stedman, 514; for the removal of the superior maxillary bone by Mr. Scott, 604; of lithotomy by Mr. Corbin, 618; of lithotrity by Baron Heurteloup, 619, 830; of amputation by Mr. Brodie, 694; rhinoplastic, for a new nose, Mr. Costello's paper on, 699; of lithotomy at the London Hospital, 748; of amputation above knee, at the Sheffield Hospital, 809; of tying hemorrhoids at St. George's Hospital, 927; for indurated strictures, 928; for aneurism, 928; for fistula in ano, 928; for fungus hæmatodes of the hip, 939; for hydrocele, 939; for cancer labii, 939.
O'Shaughnessy, Dr. W. B., review of his translation of Lugol on Iodine in scrofulous diseases, 73; proposal by, of a new method of treating cholera, 366; letter from Sunderland by, on the cholera in the north of England, 401, 459; experiments of, on the blood in cholera, 490; Report of, on the Chemical Pathology of the Malignant Cholera, review of, 929.
Opium, remarks on the primary and secondary effects of, 437; new method of detecting, 712.
Opium-eating, Dr. Christison on the effects of, on health and longevity, 614; Mr. Mart on the effects of, with cases, 710.
Os uteri, excision of, 35.
Ossification of the whole of the joints, 27.
Osteo sarcoma, Mr. Syme on, 34; extirpation of, by Mr. Earle, 378.
Ovarian disease, case of, with remarks on the only cure for, 89.
Ox, anatomy and diseases of the (*see Youatt, Mr.*)
Oxalic acid, test of arsenic by, 97.
Oxygen gas, inflation with, in asphyxia, 23.
Oxygen, on the treatment of cholera by the inhalation of, 364.

P

Paget, Dr., on malformations of the heart, 36.
Palpitation of the heart, causes and treatment of, 588.
Paralysis, Prof. Elliotson on cases of, 133, 729; Dr. Billing on cases of, 723.
Parkinson, Mr. J. W. K., remarks by, on physiological pursuits, 130.
Parr, Dr., paper on the phrenological developments of, 888.
Pattison, Mr. G. S., letters of, in reply to Dr. A. T. Thomson and the editor of THE LANCET, 82, 209.
Pectoriloquy, Prof. Elliotson on the phenomena of, 923.
Pelvis, fractured, Mr. Merry's case of, 115.
Penneck, Dr., on the nature and treatment of cholera, 798.
Peripneumony, Dr. Billing on cases of, 330, 331, 424, 583.
Peripneumonia notha, case of, 504.
Perspiration, blue, extraordinary case of, 105.
Philip, Dr. W., discussion on a pamphlet of, at the Westminster Medical Society, 663, 773.
Phrenological Society of London, paper read at, by Dr. Elliotson, on late attacks on phrenology, 287, 357; cases of loss and gain of memory, communicated to the, 410; paper on cannibalism read at, 488; paper by Dr. Elliotson on the phrenological developments of Bishop and Williams, read at, 532, 603; paper on the phrenological developments and life of Dr. Parr, read at, 888; paper on dreaming and somnambulism read at, 919.
Phthisis, mitigation of, during pregnancy, 118; Prof. Elliotson on cases of, 113, 500; Dr. Sharkey on the successful employment of the hydriodate of potash in, 764; Dr. Wood on the causes and treatment of, 915; Prof. Elliotson on auscultation in, 926.
Physicians and druggists, on the connexion between, 100.
Physiology, reflections on the study of, 130.
Piles, Mr. Brodie on cutting, 928.
Pin, passage of one, through the intestinal canal, 295.
Placenta, expulsion of, before the birth of the child, 119; Mr. Millington's case of, 232.
Plates, anatomical, uses of, 808.
Pleuro-peripneumony, lecture on, 430, 583.
Poisoning by sulphuric acid, treatment of, by magnes. carbon., 436.
Polypodium, efficacy of, with digitalis, in epilepsy, 45; Dr. Sharkey on the efficacy of, with digitalis in epilepsy, 650.
Poor, dreadful state of in London, 767, 828.

Porter, Dr. W. F., on the production of pustules on the tongue, by the tartrate of antimony, 46.
Prescriptions, various circumstances which should influence the terms of, 909.
Prolapsus glossæ, cured by compression, 489.
Pulse of cattle, where to search for, 575.
Purgatives, different effects produced by, according to the rank of the patient in society, 910.
Pus, unparalleled collection of, in the liver, 756.
Pustules on the tongue, production of, y the tartrate of antimony, 46.

Q

Quarantine regulations, propositions for a new code of, 595.
Quinine, remedy for the bitter taste of, 838.

R

Rabies in the dog, distinction of, from distemper, 581.
Ramsbotham, Dr. J. H., note from, 636.
Recto-vaginal fistula, cure of, 35.
Rectum, imperforate, remarks on a case of, 162; neglect of remedies by, 829.
Reed, Mr. W., case of monoclepsis by, 167.
Rees, Mr. H., on the nature and treatment of croup, 761, 874.
Rees, Mr. J., case of lithotrity communicated by, 830.
Reviews of, the Edinburgh Medical and Surgical Journal for Oct. 1831, 34; Ryan's Manual of Medical Jurisprudence, 137, 221, 226; Heurteloup's Principles of Lithotrity, 170; Kennedy on the Contagious Cholera, 175; Cox's Synopsis of the Bones, Ligaments, and Muscles, 285; Bransby Cooper's Lectures on Anatomy, 285; Neale's Researches into the Doctrine of Animate Contagions, 317; Forster's Essay on the Cholera Morbus, 320; Searle on the Cholera, 323; Ewertz on the Cholera, 323; A Treatise on Injuries of the Head, 405; Cragie's Elements of Anatomy, 406; Mantell's Rules in the Cholera, 407; Boyle's Medical Account of the West Coast of Africa, 487; Lizars' System of Anatomical Plates, 487; Cox's Maingault's Illustrations of the Amputations, 488; the Edinburgh Medical and Surgical Journal for Jan. 1832; the Cholera Gazette, No. 1, 594; Dr. Henry's Paper on the Disinfecting Powers of Heat, 601; Fife's Translation of Coster's Manual of Operative Surgery, 625; Hargrave's System of Operative Surgery, 625; a Catechism of Phrenology, 709; Weber's Anatomical Atlas, 709; Syme's Principles of Surgery, 736; Burt's Illustrations of Surgical Anatomy, 808; Chapman's Atlas of Surgical Apparatus, 906; O'Shaughnessy's Report on the Chemical Pathology of the Malignant Cholera, 929.
Reporting medical debates, remarks on, 768.
Respiration, obstructed, treatment of, 23.
Rheumatic neuralgia, Prof. Elliotson on a case of, 433.
Rheumatism, Prof. Elliotson on cases of, 328, 396, 501; Dr. Billing on cases of, 585; metastasis of, to the heart, 586, 589; acute, Mr. Fox's case of diseased heart following, 823.
Rupia, Prof. Elliotson on a case of, 852.
Ryan on Medical Jurisprudence, review of, and remarks, 137, 221, 222, 224; remarks on some falsities of, relating to Prof. Elliotson's clinical lectures, 298.

S

Saline agents, Dr. W. Stevens on the efficacy of, in West India fevers (*see Stevens, Dr.*); trial of, by Prof. Elliotson, 725; Dr. Knight on the efficacy of, in typhus fever, 758, 781.
Salivation, bloody, case of, after fumigation, 347.
Salmon, Mr. F., case of stricture in the colon by, 881.
Sanders, Dr., on the nature of modified small-pox, 542.
Sanson, Mr., case by, of scirrhous breast in a male, 490.
Scarlet fever, history of, at St. Bartholomew's Island, 515.
Sciatica, Dr. Billing on a case of, 431, 583.
Scirrhus uteri, trial of the potass hydriod. in a case of, 727.
Scrofulous diseases, Lugol on iodine in, 73.
Searle on Cholera, review of, 323.
Secale cornutum, efficacy of, in uterine hæmorrhage, 115.
Serum, unusually rapid effusion of, 827.
Sex of patients, influence it should have in prescribing, 960.
Shadwell, remarks on an inquest in a supposed case of cholera at, 621; letter from Mr. Mason relating to the inquest at, 712.
Sharkey, Dr. P., on the efficacy of digitalis in epilepsy, 650.
Sharkey, Dr. E., on the efficacy of hydriodate of potash in phthisis pulmonalis, 764.
Sharpe, Mr. J. B., letter from, 866.

Sheep, anatomy and diseases of (*see Youatt, Mr.*)
Sheffield Hospital, case of amputation above the knee at, 809.
Sheldrake, Mr. T., on friction in cholera, 491; communications of, with the Central Board of Health, 898; on the stealing of bodies from the Westminster Hospital, 828.
Sheriffs, Mr. S. B., new method by, of dividing the facial nerve, 632.
Ships, particular effect of fear in predisposing to disease on board of, 914.
Silvester, Mr. C. H., case of unusually rapid effusion of serum by, 827.
Simpson, Mr. W., case of hydrophobia by, 29; remarks by, on Mr. Levison's case of glanders in the human subject, 99.
Sinapisms, preparation of, with water, 838.
Skull, Mr. Fox's case of fracture of, 48.
Small-pox, its identity with cow-pox established, 541; remarks by Dr. Howison on, 540; modified, Dr. Sanders, of Edinburgh, on the nature of, 542; Dr. Negri's case of malignant, 629.
Smith, Dr. G., on an inquest at Lisson Grove, 297; on the late murders by burking, 439.
Soda, Mr. Lyon's experiments on the blood with the muriate of, 655.
Somnambulism, explanation of the phenomena of, 919.
Southwark, cases of cholera at, 741; miserable condition of the poor in, 826.
Spine, Prof. Elliotson on a case of injury to the ligaments of, 131; other cases of injury to the, 166, 196.
Spleen, Prof. Elliotson on disease of, 196; extraordinary enlargement of, in horses, 872.
Stedman, Dr., extirpation of a scirrhous tumour by, 514.
Steel fork, Dr Burne's case of extraction of, from a sailor's back, 662.
Stein, Mr. J. H., on the treatment of cholera by inhalation of oxygen, 364.
Stethoscope, Dr. Nagle on the use of, in pregnancy, 447.
Stevens, Dr., charge made by Dr. Johnson against the experiments of, on the blood, 376; reply of, to Dr. Johnson, 412; on the efficacy of saline agents in West India fevers, in reply to the charges of Drs. Hacket, Johnson, and others, 553; reply of Dr. Johnson to, 591; rejoinder of Dr. Johnson, 657.
St. Bartholomew's Hospital, case of imperforate rectum at, 72; extirpation of the superior maxillary bone at, 378; bad arrangements during the operations at, 379; shameful mode of conducting the post-mortem examinations at, 434.
St. Bartholomew's Island, history of the scarlet fever at, 515.
St. George's Hospital, list of medical schools near, 16*; clinical lectures of Mr. Brodie at, on cases of disease at (*see Brodie, Mr.*)
Stodart, Mr. E., on the characters which prove cholera to be a new disease in England, with cases, 867.
Stomach, connexion of, with other organs, 760.
Stokes, Dr., case of fistula in the abdomen attended by, 612.
Strains, effect of, in producing aneurisms, 591.
Strangulation, treatment of cases of, 26; cause of certain effects produced by, on the genital organs, 49, 50, 51, 98, 99, 661.
Stricture in the colon, Mr. Salmon's case of, 881; indurated, of the urethra, Mr. Brodie on, 921.
St. Thomas's Hospital, medical schools near, 15*; Prof. Elliotson's connexion with, 64; clinical lectures delivered by Prof. Elliotson on cases occurring at, 131, 162, 196, 309, 324, 390, 432, 469, 498, 586, 689, 725, 754, 850; supposed case of ruptured femoral artery at, 134; case of injury to the spine at, followed by incontinence of urine, 166; bad arrangements at the operations at, 229; case of bloody salivation after fumigation at, 347.
St. Vitus's dance, in boys, Prof. Elliotson on cases of, 392; case of, at the Westminster Hospital, 458.
Students, medical and surgical, address of the Editor to, on their entering to the London hospitals and lectures, 1, 51; mode by which they are entrapped into paying fees, 3; cautions to, against throwing away their money, 5, 16*; arrangements of their time between the schools, 14, 51; advice of Mr. S. Cooper to, 62; of Prof. Elliotson to, 69; suggestions to, for preventing the neglect of teachers, 95; objections to the admission of to the British Lying-in Hospital, 443; unjust conduct of the Dublin Apothecaries Company to, 748; dinner given by, at the London University to their professors, 812; complaints of (*see the hospitals*).
Styptic, alleged discovery of a new, by Messrs. Talrich and Halma Grande, 522, 523, 620.
Sulphur, external application of the chloride of, in cutaneous affections, 46.
Suffocation, pathology and treatment of, 23.
Sunderland, Dr., identity of small-pox and cow-pox established by, 541.
Surgery, introductory address of Prof. S. Cooper upon the principles and prac-

tice of, delivered at the London University, 56; review of Syme's Principles of, 736.
Sweetmeats, coloured, poisonous nature of, 843.
Swine, anatomy and diseases of (*see Youatt, Mr.*).
Syme, Mr., on cases of fracture of the leg, 33; on cases of lithotomy, 34; osteo sarcoma, recto-vaginal fistula, ulcer of the nose, 35; and nervous tubercle, 36; review of his Principles of Surgery, 736.
Synocha petechialis, Dr. Billing on cases of, 645, 721.

T

Taylor, Mr. J., case of vaginal hernia by, 204; case of partial loss of speech from a blow on the head by, 508.
Taylor, Dr. G., on the proposal of, to open the radial artery in cholera, 365, 744.
Temperament, importance of regulating doses according to, 910.
Testitis, efficacy of mercurial ointment in, 47.
Tetanus from a puncture of the foot, case of, 715.
Thomas, Mr. J., essay on asphyxia by, 23; synopsis of the treatment of cholera by, 709.
Thomson, Dr. A. T., Mr. Pattison's reply to, 82.
Tic doloureux, cure of, by the cyanuret of potassium, 873.
Times, The, its decision against the cholera in London, 777, 808.
Tomkins, Mr. Wm., case of obstructed labour by, 747.
Tonics, effect of erysipelas, 584.
Tongue, prolapsus of, cured by compression, 489.
Toothach, cure of, by alum, 611.
Tracheotomy, operation of, in the horse, ox, &c., 781.
Tubercle, nervous, of the leg, 36.
Tumour, scirrhous, removal of, by Dr. Stedman, 514.
Turpentine, case of melena successfully treated by, 818.
Turner, Prof., reply of to the letters of Mr. Pattison, 185.
Typhus fever, Dr. Knight on the use of alkaline salts in, 758, 785.
Tyrrell, Mr., letter of Dr. Blicke to, 858.

U

Ulcer of the nose, case of, 35.
University of London, advantages of attending, 16; introductory lecture to a course on surgery by Prof. Cooper at, 50; introductory lecture to a course on medicine by Prof. Elliotson at, 61; want of a hospital at, 64, 66; controversy connected with, between Mr. ex-Prof. Pattison, and Drs. Thompson and Turner and the editor, 82, 185, 209; announcement of the publication of Mr. Youatt's lectures on veterinary medicine at, 337; Mr. Youatt's course of lectures at, 349, 353, 381, 386, 413, 418, 461, 466, 493, 525, 528, 573, 578, 605, 637, 641, 685, 717, 749, 781, 813, 845, 877; dinner given to the professors of, by the students, 812; establishment of a senatus academicus in, 839; charter of, not forthcoming, 840; desertion of, by in-place whigs, 840; proposal to found a hospital at, 841.
Urine, albumenous, Dr. Gregory on the connexion of, with renal disease, 36; case of incontinence of, arising from injury to the spine, 166.
Urticaria, Prof. Elliotson on a case of, 432.
Uterus, rupture of, without vomiting, 830.

V

Vaccination, remarks by Dr. Howison on, 540.
Valerian infusion, Sir H. Halford's mode of prescribing, 928.
Veterinary medicine and surgery, course of lectures on, at the University of London (*see Youatt, Mr.*).
Viper-bite, Mr. James's case of, 294.
Vomiting, suppression of, by prussic acid, 47; rupture of the uterus without, 830.

W

Wallace, Mr., claims of, to the discovery of the disinfecting powers of heat, with an account of his apparatus used in cases of cutaneous diseases at the Skin Infirmary, Dublin, 854.
Waring, Mr. E., letter from, respecting the London College of Medicine, 31; remarks by, on the use of prussic acid in vomiting, 117.
Watson, Dr., conduct of, at the London University, 842.
Weatherill, Dr., on a scandalous inquest at Liverpool, 746.
West, Mr., efficacy of the mag. carb. prepared by, in poisoning by sulphuric acid, 437; Mr. R. U., on the primary and secondary effects of opium and other narcotics, 437.
Westminster Hospital, medical schools near, 16*; case of femoral aneurism at, 102; ligature of the external iliac artery at, 103; operation of lithotomy at, 135; introductory address of Mr. Edwards at, to a course of clinical demonstrations, 143, 332, 444; surgeons'

levee at, 229; case of stone in training for lithotrity at, 233, 313; hole-and-corner proceedings at, 231; case of aneurism of the external iliac artery at, 314, 347, 457; partial mode of electing clinical clerks at, 338; case of injury to the hand and arm at, 346; cases of aneurism of the cœliac axis at, and chorea sancti viti, 458; neglect of a patient at, 459; case of tetanus at, from a puncture of the foot, 715; fatal case at, of wound of the orbit, 715; clandestine removal of dead bodies from, 810, 828.

Westminster Medical Society, papers and discussions at, on the mode of spreading of the malignant cholera, 299; on the nature and treatment of the cholera, with propositions by Dr. Johnson, 340, 407; on Dr. O'Shaughnessy's proposal for treating the cholera by injections of highly oxygenated salts, 366; on the anatomy question, 410; on the cholera, continued, 444; erratum in a speech at, 523; paper on phrenology read at, 632; remarks at, on Dr. W. Philip's pamphlet on cholera, and on the anatomy petition, 663, 773; Mr. Costello's paper at, on a new rhinoplastic operation, 699; exhibition of Dr. Auzoux's anatomy model at, 703; on the advantage of combining quinine with alkalies, 732; Dr. Gregory's "obscure" case, at 733; renewed controversy on cholera at, 768; continued, with Dr. Epps's table of cholera cases, 802-3, 869, 895; paper read at, on the epidemic cholera of Leeds in 1825, 894.

Weber's Anatomical Atlas, review of, 709.

Whig nobility, their desertion of the London University, 840.

Whittaker, Mr. J. S., on the nature and treatment of croup, 836, 874.

Williams, Dr., views entertained by, on the nature and treatment of continued fever, 183, 294.

Williams and Bishop the burkers, phrenological developments of, 532.

Winslow, Mr. F., remarks by, on the miserable condition of the poor in Southwark, 826.

Wisbey, Mr. H., case of ruptured uterus by, 830.

Wood, Dr. T., on the causes and treatment of phthisis, 915.

Worms in the alimentary canal, Prof. Elliotson on, 390.

Wright, Mr., on the treatment of deafness by galvanism and electricity, 87.

Y

Yellow fever, effect of miasmata in producing, 914.

Youatt, Mr., course of lectures by, on veterinary medicine and surgery, delivered at the London University, 337, 377:—Introductory lecture to a course of medicine, 78; division of the subject; preliminary remarks on the brain, the nerves, and circulation of the blood; respiration, and the importance of its influence over the animal economy; structure of the nasal cavity of the horse, 349 to 352. Lecture 2—Nasal cavity of the ox, sheep, swine, and dog; fracture of the nasal bones of the horse, its cause, effects, treatment, and prognosis; with cases, 353 to 356. Lecture 3—The superior and anterior maxillary bones of brute animals; the palatine bones and the vomer; the septum and æthmoid bones; the turbinated bones; the meatus, the maxillary, and other sinuses, and the lachrymal ducts; fistula lachrymalis, 381 to 386. Lecture 4—The nostrils and their muscles; difference of the nostrils in horses; the membranes generally, and the Schneiderian membrane in particular, 387 to 390. Lecture 5—The veins and nerves of the nose in the horse, &c.; spontaneous bleeding from the nose in the horse and ox; the sense of smelling, 413 to 417. Lecture 6—Polypi in the nose of the horse, ox, and dog, and its removal by operation; coryza and nasal gleet in the horse, 418 to 423. Lecture 7—Glanders, its symptoms and post-mortem appearances; its division into acute and chronic; greater frequency with which the left nostril is affected; progress of the disease from glanders to farcy; diseases often mistaken for glanders; affection of the lungs in glanders, 461 to 465. Lecture 8—First seat of glanders; general view of the disease, its causes, &c.; absurd and cruel system of English stabling, and its fatal effects, 466 to 468. Lecture 9—Causes of glanders continued; want of proper exercise; liability of the horse to this disease; on glanders running in the stock; its production by damp air; effect of age on the disease; Is glanders contagious? proofs; its mode of communication; varying period of its appearance; duty of veterinary surgeons; Does glanders ever occur in man? precautions in case of glandered horses, 492 to 498. Lecture 10—The treatment of glanders; supposed cases of; effect of pure atmosphere and turning out; local applications to the disease; counter irritants; sulphates of copper and iron; tricks of horse dealers, 525 to 527. Lecture 11—Farcy; disease of the absorbents; connexion of farcy with glanders; diseases resembling farcy; Is farcy conta-

gious? its local and constitutional treatment; apparent cures; recurrence of the disease, 528 to 532. Lecture 12—Ozena in the horse, its treatment, and distinction from other diseases; worms in the horse's nose; coryza in cattle, and its treatment; glanders and farcy in cattle; coryza in sheep, and its treatment, 573 to 577. Lecture 13—Coryza and ozena in dogs; worms in the dog's nose; distemper, its contagionsness; varieties, symptoms, progress, and treatment; distinction between distemper and rabies, 578 to 581. Lecture 14—Distemper in the dog continued, 605. Lecture 15—Nasal catarrh in the cat, rabbit, and poultry; the velum palati; the os hyoides of the horse, ox, dog, and swine, 637 to 639. Lecture 16—Strangles in the horse and ox; swellings under the throat in dogs, 641 to 644. Lecture 17—Anatomy of the larynx in the horse, ox, dog, and swine, 685. Lecture 18—The larynx continued, 717. Lecture 19—The thyroid glands and trachea, 749. Lecture 20—The respiratory canal; suffocation from disease; mode of performing tracheotomy in the horse; employment of respiratory tubes; tracheotomy in cattle and dogs; the thymus gland, its anatomy, functions, and physiology; the bronchial tubes, 781 to 784. Lecture 21—The thorax of the horse; the ribs, sternum, and intercostal muscles; proper form of the thorax; evils of tight girthing, 813 to 818. Lecture 22—Anatomy of the diaphragm; spasm of the diaphragm; openings through that muscle; arteries, veins, and nerves of the diaphragm; Sir Charles Bell's experiments; mechanism of respiration; inflammation and rupture of the diaphragm, 845 to 849. Lecture 23—Serous membranes; the pleura and its surface; the surface of serous membranes, and the fluid exhaled by them; characters of serous membranes; the pleura and its purposes; opposite effects of purging in pneumonia and pleurisy in the horse; connexions of the pleura and the mediastinum, 877 to 880.

Z

Zeucken, Dr., case of cholera treated by, 216.

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